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PHILOSOPHICAL
OBSERVATIONS
ON THE
SENSES
OF
VISION AND HEARING;

TO WHICH ARE ADDED, A TREATISE

ON HARMONIC SOUNDS,

AND AN ESSAY

ON COMBUSTION

AND

ANIMAL HEAT.

538 f 13 / 2

BY J. ELLIOTT, APOTHECARY.

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MVSEVM
BRITAN
NICVM

TO

SAMUEL FOART SIMMONS, M.D.
F. R. S.
MEMBER OF THE
ROYAL COLLEGE OF PHYSICIANS,
LONDON,
AND ONE OF THE FOREIGN FELLOWS
OF THE ROYAL SOCIETY OF MEDICINE AT
PARIS;
A GENTLEMAN
NOT LESS DISTINGUISHED BY
HIS HUMANITY
AND SUPERIOR SKILL IN HIS PROFESSION,
THAN
BY HIS EXTENSIVE ACQUAINTANCE WITH, AND ZEAL
TO PROMOTE EVERY BRANCH
OF USEFUL KNOWLEDGE.
THE FOLLOWING SHEETS
(AS A SMALL, BUT SINCERE TRIBUTE TO HIS MERIT)
ARE RESPECTFULLY INSCRIBED
BY HIS OBLIGED,
AND DEVOTED HUMBLE SERVANT,

Carnaby Market,
Nov. 4, 1779.

J. ELLIOTT.

E R R A T A.

Page iv. line 13, for *chin* read *upper lip within the face.* Page xii. line 4, for *is* read *be.* Page xx. line 7, for *excite* read *excited.* Page xxiii. line 19, for *at* read *of.* Page lxi. line 14, for *mean* read *reſult.* Page lxxviii. line 1, for *of* read *for.* Page lxxxviii. line 10, for *inflammable* read *uninflammable.* Page cxli. line 18, for *with the bodies* read *with bodies.* Page cxix. line 10, after *air,* read *more than by one another.* Page clxxxv. line 14, omit *more.* Page clxxxvii. line vi, for *philologiſts* read *phyſiologiſts.* Page cxci. line 12, for *walking* read *waking.* Page ccxi. line 2, after *that* read *either atmoſpherical or dephlogiſticated. See alſo page* cxi, &c.

Note. To what is ſaid in the Appendix relative to the four principles, may be added, that particles of fire ſeem to attract æther more than they do one another, and more than they do particles of earth. And that particles of phlogiſton attract æther more than they do one another, but leſs than they do particles of earth. The reaſons for this opinion will eaſily be perceived by thoſe who read the laſt Eſſay.

PREFACE.

THOSE who have any acquaintance with the Preſs know that the firſt ſheet of a book, containing the title page, &c. is generally printed laſt. This remark may account for ſome paſſages in the ſequel.

THE firſt of the following Eſſays I have had by me many years, and have my reaſons for mentioning that it is taken from a folio manuſcript containing many other inquiries, which was in the hands of perſons, not my friends, ſo long ago as the year 1772; the greateſt part of that manuſcript was written long before: and the ſubſtance of the Eſſay alluded to, has been in the poſſeſſion of a reſpectable philoſophical character near three years, though it has not till now been convenient for me to make it public.

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IN the ſecond Eſſay, I forgot to mention that the note 1, in the ſcale, page 79, for the violin, may be called C; and ſo in proportion for the larger inſtruments; or elſe the name may be varied by tuning higher or lower, as mentioned in the Eſſay. The ſtrings fitted to the violin may be very ſmall fourths, or good thirds of the leſſer ſize. I once fitted a guittar with ſix, and afterwards with eight ſtrings, regularly tuned on the idea of that ſcale, and convinced myſelf that the hint might be proſecuted to advantage on viols. By tuning eight ſtrings according to the muſical intervals of an octave, the compaſs of a fifteenth may be obtained, without having recourſe to any of the difficult frets, or leſs melodious notes. And the ſame compaſs (which is ſufficient for moſt performances) may be commanded with only ſix ſtrings, by taking in notes above the one fourth fret, which notes will ſtill be good, and not difficult to hit. Six ſtrings can eaſily be managed with the bow: for greater nicety in the execution, the frets may be marked; and perhaps it will be found more convenient to uſe the notes on the two-fifth diviſion, inſtead of thoſe on the one-fifth, as there will then be no occaſion to ſhift the hand. An inſtrument of this kind might either be played by itſelf, or uſed to ſtrike in the harmonic notes occaſionally in a concert, &c. where their effect would be much finer, than when only drawn from a common inſtrument tuned fifths, as would be found on trial. I did not think to inſert a ſcale for the viol di gamba; but it may eaſily be conſtructed by thoſe who underſtand the theory. It may be remarked, that octave notes *on the ſame ſtring*, are rather *bearing;* and the like may be obſerved of the other intervals: the reaſon will be obvious to thoſe who read that Eſſay.

In regard to the laſt Eſſay, I have been a little unfortunate, as a moſt excellent treatiſe has been publiſhed on the ſubject by Mr. Crawford ſince mine was in the preſs *: thoſe who have read that truly admirable work, will therefore find ſome errors, which otherwiſe perhaps they would not have known to have been ſuch, at leaſt for the preſent. My performance may be compared to the firſt dawn of twilight; Mr. Crawford's to meridian ſunſhine: and it is rather an unlucky circumſtance that the *latter* ſhould have appeared before the *former*. Yet it ought to be remembered how highly Des Cartes' philoſophy was admired, even by the moſt learned, before Newton's Principia appeared. It may be added, that Mr. Crawford had the advantage of being able to proſecute the inquiry by experiments; whereas I, having been leſs eligibly ſituated in life, could only proceed by mere ſpeculation, or gueſs-work.

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* It was no fault of the Author (as could be made appear were it neceſſary) that this work was not publiſhed in June laſt.

For the ſame reaſon, I have not been able to pay that attention to the ſtyle which is neceſſary to a work intended for the Public. The ill-natured critic will aſk, why then did I publiſh at all? I anſwer, that I ſhould not have troubled the world with theſe Eſſays, if better judges than me had not thought that they contained hints which thoſe who have leiſure and proper conveniencies might improve into real diſcoveries: a ſection * in Dr. Prieſtley's celebrated Hiſtory of Electricity will ſufficiently juſtify my conduct in this reſpect.

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* Part IV. ſection I.

PHILO-

OBSERVATIONS
ON THE
SENSES.

SECTION I.

OF VISION.

THE uſes of the ſeveral humours and coats of the eye, and the manner in which light is refracted by them for forming the images of objects on the retina, have already been explained by philoſophers. My deſign is, to endeavour to ſhow in what manner light acts after it arrives at the retina, for cauſing the appearance of lumination or colour. This branch of optics has alſo been cultivated; but as a conſideration of what happens in this ſenſe, will ſerve to illuſtrate what I have to advance concerning the ear, I ſhall enlarge on it, regardleſs of what may already have been written on the ſubject, eſpecially as my obſervations are of a new kind; or at leaſt have not hitherto been ſo fully attended to.

It has been frequently obſerved, that if the corner of the eye be preſſed with the finger, a luminous ſpot or ring will appear, ſomewhat differently coloured; and that from a ſtroke on the eye, an appearance like a flaſh of fire has ſometimes been obſerved.

After repeated trials, and putting myſelf to ſome pain, I learnt that by preſſing the balls of my eyes with my hands, in the direction of their axes, with as much force as I could bear, keeping them ſteady, and aſſiſting the preſſure with the ſtrong compreſſion of the lids, and contraction of the neighbouring muſcles, there would, after ſome time, appear a large luminous ſenſation like a concave hemiſphere of light, but not very lucid, and chequered (often in a very regular manner) with dark and leſs lucid intervals. If the preſſure be continued, and the eyes winked very ſtrongly, the appearance will be much brighter, and will ſeem to tremble. There will ſometimes alſo appear large crooked ſtreaks of light, much brighter than the other parts, and with certain vermicular, or eel-like motions. By increaſing the preſſure till the eyes become quite hot and red, the light will be at the brighteſt, and almoſt as lucid as at noon day: till this time the appearance is generally of a whitiſh colour, tinctured with yellow, or orange, like the ſun; or rather like the light of the moon, or a candle. By continuing the extreme preſſure, the brightneſs of the appearance begins to decay, and the colour gradually changes from a reddiſh and yellowiſh white, to a bluiſh one; and ſometimes ſeveral kinds of coloured ſpots will appear, as red, green, blue, and a fine violet which generally diſappears the laſt, and thoſe more verging to red ſooneſt; for now the light totally vaniſhes, nor can it be recalled by a continuation or increaſe of the preſſure. If now the hands be removed, and the eyes opened, they will be quite blind even to the direct light of the ſun; and it will be ſome time before they recover their ſight, and then but by degrees.

THIS experiment is very painful, and it is not every one that would chooſe to repeat it after me with the requiſite care. Before I proceed farther, it may be neceſſary to clear up ſome particulars which are to be met with in the above account.

The luminous appearance ſeems like a concave hemiſphere, or parabolic conoid; it ſeems to ſurround the face only, and not the whole body. For example, if I preſs the corner of either eye, a ring of light will appear on the ſide of the face oppoſite thereto; and by removing the finger at intervals all round the margin of the eye, the ring will appear to move all round the face: that is, when the finger is on the right ſide of the eye, the ſpot or ring will appear on the left ſide of the face; and contrariwiſe. Alſo, when the finger is above the eye, the ſpot appears as if it was about the chin; and when the finger is under the eye, the ſpot appears as if upon the forehead; but in all theſe caſes, provided the edge or margin of the retina be preſſed, it ſeems as if it was cloſe to the face. So when the lumination is excited in the manner above deſcribed, the margin of it ſeems to touch the face, but the centre of it appears as at ſome diſtance from the face, and the diſtance is leſs as you approach towards the margin; which gives the whole the reſemblance of a concave ſegment of a ſphere, or parabolic conoid, as mentioned above. Alſo the two eyes form but one hemiſphere, as I know by cauſing this lumination by preſſing one eye only. Now as theſe are affairs (I think) merely mechanical, or reſulting from organization, and in which I could not have been miſled by cuſtom or habit, as is the caſe in many inſtances of common viſion, theſe appearances having occurred at the very firſt time of exciting the above lumination, may they not be made uſe of to ſettle the famous diſputes concerning the inverſion of images in the eye, and ſeeing ſingly with both eyes? Has not every part of the retina of one eye an anſwerable part in the other? Do not the correſponding fibres of the right ſides of both eyes meet in the brain, and terminate in the left ſide of the ſenſory? Thoſe in the left ſides of both eyes, in the right ſide of the ſenſory? and thoſe in the upper and lower parts of the retinæ of both eyes in the contrary parts of the ſenſory, ſo as to be in an inverted ſituation in the latter, to what they are in the former *?

BUT

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* The moſt deciſive experiment that I have met with againſt the junction of the reſpective fibres of both eyes is that blue and yellow thrown ſeparately on the anſwerable parts of both retinæ

BUT it is moſt worthy of remark, that by preſſing the margin of either eye, the ring appears not as on the oppoſite ſide of that *eye*, but of the *face*. Alſo the concavity of the luminous appearance which ariſeth on preſſing the centre of the eye, and its ſurrounding not the *whole body*, nor merely the *eye*, but the *face*, ſhew that the retina in the brain encompaſſes the whole of that portion of the ſenſory which anſwers to the *face*.

THE luminous appearance which ariſeth on preſſing the centres of the eyes, as deſcribed

retinæ do not cauſe a green. The correſponding fibres are not perhaps, united, becauſe then if one nerve was diſordered or deſtroyed, the other would alſo. They may only run by the ſides of each other, or even in contrary directions, and terminate in the brain ſo as to form two different ſurfaces, concentric, and at a ſmall diſtance from each. And this ſeems to appear by certain circumſtances of the chequers in preſſing both eyes; in other reſpects the opinion ſeems to be well founded. Thus any two anſwerable parts of the eyes, whether excited by the rays of light, or by preſſing the corners or centres, yield but one ſenſation, as if but one eye had been affected; excepting that the ſenſation is ſtronger. In ſquinting eyes the ball is perhaps diſtorted with reſpect to the retina, which may account for any ſeeming deviation, in theſe inſtances, from the above rule.

above, is chequered; that is, ſome parts of it are darker than others, and ſometimes there appear ſpots, and ſtreaks which are much brighter than the other parts of the lumination. The cauſes of which I take to be that the ſurface of the retina is not even or ſmooth, but has prominencies or ridges anſwerable to the regular form of the chequers, and which may reſult from its ſtructure *; ſo that the vitreous humour muſt preſs on it unequally, and by that means cauſe ſome parts of the appearance to look brighter, and others darker: for the cauſe of the lumination is in the retina, as will preſently be ſhewn. The apparent trembling I take to proceed from the trembling of the eye and retina, on account of the violent preſſure; the moving vermicular ſtreaks may ariſe from the convulſion of the membranes, or coats of the eye impreſſed on the retina, and perhaps alſo from the convulſions and tremblings of theſe parts themſelves; the blindneſs ariſeth, I ſuppoſe, from the univerſal oppreſſion of the fibres of the optic nerve expanded in the retina, and this paralyſis of them I take to be the reaſon

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* From its net-like ſtructure it derives its name (*Retina*).

why the eyes do not recover their ſight immediately, but by degrees; becauſe it muſt be ſome time before the nerves can be relieved from their oppreſſion, ſo as again to communicate the action of the rays of light on them freely to the ſenſory.

When the corner of the eye is preſſed with the finger in the uſual way, there does not appear an univerſal lumination, as on preſſing in the direction of the optic axis, but only a ſmall ring or ſpot of light about half an inch in diametre. There is alſo this difference between them, that the ſpot cauſed by the lateral preſſure preſently diſappears, unleſs the eye be ſtruck repeatedly, or with a quavering motion; whereas the lumination excited by preſſing the centres of the eyes, continues without requiring ſuch quavering motion. The reaſon of this difference I take to be, that in the firſt caſe only a ſmall part of the globe of the eye is protruded on the retina by the oblique preſſure, and of courſe only a ſmall ſpot, proportional thereto, is excited in the ſenſory. The reaſon why it ſo ſoon diſappears is, I take it, becauſe the globe being ſoft, and preſſed but obliquely,

liquely, gives way, and changeth its figure ſo as to make the preſſure on the retina equal; whence the partial preſſure made by the protruſion on the retina there being removed, the luminous ſpot diſappears; but if the finger be lifted up, or the preſſure leſſened, the globe preſently recovers its figure; and if ſtruck by the finger again, the ſame partial protruſion, and luminous ſpot ariſing therefrom will be cauſed, but which will again preſently diſappear, by reaſon of the globe changing its figure as above; and hence the neceſſity of the quavering motion of the finger to preſerve the ring *. But theſe things do not happen on preſſing the centre of the eye, becauſe there is then no room for the globe to elude the preſſure, which in this caſe is therefore general; ſo that the lumination once excited, continues as long as the preſſure remains,

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* By preſſing the corner of the eye harder than uſual, I have ſometimes excited two rings, one on each ſide of the face; that at the ſide oppoſite to the corner preſſed aroſe in the uſual way; the other, I imagine, from the effect of the preſſure paſſing diametrically through the globe, and cauſing a protruſion on the oppoſite part of the retina.

mains, or till the nerves become paralytic. And even if the lateral preſſure be continued and increaſed for ſome time after the luminous ring diſappears, there will ariſe an univerſal lumination, as on preſſing the centre of the eye with the ball of the hand, and for the ſame reaſon. But the experiment is ſo painful that it cannot be made to advantage.

THESE experiments are beſt made in the dark and in bed. Before and after ſleep, or fainting, the lumination deſcribed above appears in ſome meaſure even without preſſing the eye. Luminous ſparkles are alſo perceived by a perſon in a dark room, cauſed merely, I ſuppoſe, by the action of the circulating fluids, &c. on the retina.

Now the general concluſion which I would draw from what has been ſaid is, "That colours are liable to be excited in the eye which do not at all depend on the rays of light." A concluſion which I imagine no perſon, who properly conſiders the experiments, will refuſe me.

FROM the analogy diſcovered by Sir Iſaac Newton between the vibrations of the rays of light, and thoſe of muſical ſtrings, or of the pulſes of air for cauſing muſical ſounds, it is concluded that theſe rays cauſe viſion by means of their vibrations; and that the different colours, like notes of muſic, depend on the different times of the vibration: may we not therefore infer, "that ſince colours may be excited in the eye, independent of the pulſes of the rays of light, they are cauſed by vibrations liable to be excited in the eye, of the ſame times as thoſe of the rays of light? And that as there are different kinds or tones of colour, there are alſo as many different times of vibration for cauſing them?" For the luminous appearance which ariſeth on preſſing the centre of the eye, is in general white like the ſun's light, or rather like that of the moon, or a candle. But ſuch a colour as this is found by refracting light to be, not an original or ſimple colour, but compounded of others; as red, yellow, green, blue, and violet, with all their intermediate ſhades anſwerable to the degrees of refrangibility. And by parity of reaſon, ſince the like white colour ariſeth on preſſing the eye, this alſo cannot be a ſimple colour, but one compoſed of the ſame ingredients; that it is ſo appears by the experiment above deſcribed; for there ſome of the ingredients appeared either ſeparate, or much leſs compounded. And even the white varies its colour, being ſometimes a reddiſh or yellowiſh white, and at other times verging to a blue. The ring or ſpot which appears on preſſing the corner of the eye, is likewiſe ſometimes variouſly coloured, as others have obſerved. Alſo if this ring be made very luminous, it is tinctured with yellow or red; but if it is faintly excited, it is rather inclined to a greeniſh blue, as is the caſe in the central preſſure. Now, by applying the old maxim of philoſophers, that "Nature does "nothing in vain," may we not be allowed to draw the following concluſions? viz. "That the rays of light could not conveniently be made to communicate their vibrations immediately to the nerves, but that the interpoſition of thoſe ſhewn to exiſt in the retina was neceſſary to that end. That therefore there are in the retina different times of vibration liable to be excited, anſwerable to the times of vibration of the ſeveral ſorts of rays. That any one ſort of rays, falling on the eye, excite thoſe vibrations, and thoſe only which are in uniſon with them, not at all affecting the others, and therefore cauſe only their proper colour. And that in a mixture of ſeveral ſorts of rays, falling on the eye, each ſort excites only its uniſon vibrations, whence the proper compound colour reſults from a mixture of the whole.

N. B. As the following remark relates to viſion, it may be here ſubjoined.

MARRIOTTE has publiſhed a curious experiment which ſhews that there is an inſenſible ſpot in the retina, at the entrance of the optic nerve. If an object be looked at whoſe image occupies the whole ſurface of the retina, one would imagine from thence that an hole or dark ſpot ſhould be perceived in the part of the object anſwerable to the inſenſible ſpot in the retina; but no ſuch ſpot or hole is ſeen. I have obſerved that images which fall near that ſpot are not perceived as properly defined. In the concave lumination excited by preſſing the centre of the eye alſo, no ſuch ſpot is diſcernible. There is no ſuch ſpot or vacuity therefore in the retina of the ſenſory (if I may be allowed the expreſſion); it ſeems to be filled up by the fibres of the optic nerve diſperſed around the ſpot in the eye: hence the ill defined images there.

SECTION II.

OF TASTE, SMELL, AND FEELING.

SINCE the diſcovery of the analogy between colours and ſounds, the various kinds of taſtes and ſmells have been conſidered as ſo many different tones or notes of theſe ſenſations. In what manner liquids and odours act on the organs for the purpoſe of cauſing the reſpective ſenſations, whether by vibration or otherwiſe I pretend not to determine. All that I have to obſerve on the ſubject is, that with regard to taſte and ſmell, the caſe is not the ſame as hath been ſhewn with reſpect to the eye: that is, "There are no innate taſtes or ſmells liable to be excited by preſſing or irritating theſe organs, as is the caſe with the colours in the eye;" at leaſt I have not been able to diſcover any ſuch by the experiments that I have made: and therefore, if odours and liquids cauſe taſtes and ſmells by means of vibrations, they muſt act immediately on the nerves.

Whether feeling, or pain, be cauſed by vibration, or in what other manner, I alſo cannot determine, though I ſuſpect the former. I have only to obſerve, that the manner in which this ſenſation is ordinarily excited is different from what happens in the eye. Viſion is uſually excited by uniſon vibrations, though it may be cauſed by preſſure or irritation, as hath been ſhewn: "but feeling or pain is ordinarily excited by mere irritation or preſſure." In which reſpect it alſo differs from taſting and ſmelling.

N. B. The ſenſes are uſually reckoned but five; I would add thirſt, hunger, heat, cold, titillation, &c. But of theſe I have nothing to ſay but what may be collected from the obſervations on taſtes, ſmells, and pain.

SECTION III.

OF HEARING.

I HAVE purpoſely deferred the conſideration of this ſenſe till laſt, becauſe there are ſome particulars of the others which will be proper for illuſtrating what I have to advance concerning this. It appears that, in the different ſenſes, different modes of operation of the reſpective agents take place. In the organs of taſte and ſmell, liquids and odours communicate their action immediately to the nerves. In feeling, or pain, nothing but irritation is employed. But viſion, though it may be excited by mere irritation, as hath been been ſhewn, and as is the caſe with the ſenſe laſt mentioned; yet for obtaining the ſhapes of objects, and for other purpoſes of ſeeing, it was neceſſary to employ the rays of light; which yet do not act, like liquids and odours, immediately on the nerves, but by the mediation of uniſon vibrations. It has, I think, been generally ſuppoſed, that the caſe with the ſenſe of hearing is ſimilar to what has been ſaid concerning taſte and ſmell, viz. that the vibrations of the air are communicated immediately to the auditory nerve. But that this is not true, may perhaps be concluded from the following obſervations.

It has been remarked by others, that when a perſon is ſleepy, or tired, when the ears are ſwelled by means of a cold, &c. before and after ſleeping, or fainting, in gaping wide, and on other occaſions; but particularly when the ears are violently ſtruck, a noiſe is ſometimes heard, called vulgarly "a ſinging or ringing in "the ears;" having obſerved it in myſelf, and particularly twice, when I heard ſeveral diſtinct muſical ſounds, I conceived the phenomenon to be worthy of notice, and proceeded to examine it. It would be tedious to recount the methods which I practiſed, and the pain I ſometimes put myſelf to in order to find out a method of exciting theſe ſounds. Let it ſuffice, that by ſtrongly contracting the muſcles on the ſides of my head, and thruſting my fingers into my ears, preſſing different parts of them, more eaſy to be learnt by experiment than deſcription, I could at any time excite them, in a confuſed medley, to a great number. And if I made the experiment when warm in bed, and inclined to ſleep, I could at length, merely by preſſing my finger on different parts of my ear, excite ſome of them in a manner ſufficiently diſtinct to be conſidered as if ſeparate or alone, and by that means make out a kind of plain tune. And even when they are excited in the moſt confuſed manner, ſome may be attended to and conſidered independent of, or diſtinct from, the reſt; as is the caſe when many ſounds are heard together in a concert. By purſuing my inquiry, I found that they had the following properties.

I. THAT theſe ſounds are not audible in the natural ſtate of the ear, but require diſtenſion or preſſure, or, more properly ſpeaking, *irritation* to excite them.

II. THAT the loudneſs or ſtrength of the ſound might be increaſed by increaſing that irritation.

III. THAT the ſame ſound never varies in tone or note;—as I knew by comparing ſeveral of them which I could excite with certainty at pleaſure, with the notes of a fixed muſical inſtrument, with which I at firſt found them in uniſon; for I could never afterwards find any ſenſible difference between them.

IV. That they do not at all depend on the pulſes of the air;—for if I excite them ever ſo ſtrongly they could not be heard by another perſon; which, from their loudneſs, muſt have happened had they been the effects of aërial pulſes.

V. That there are a great number of them.—The exact number, or latitude of their ſcale, I have not yet been able to determine, becauſe of the difficulty attending the ſubject. It requires practice and patience to excite them at all to advantage: the ears muſt be compreſſed by the muſcles around; the mouth occaſionally opened or ſhut; a gaping raiſed; the fingers preſſed not only at different parts of the hole, but alſo around the external ear *; the

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* By thruſting the finger into the ear, and then withdrawing it, ſo as to cauſe a vacuum in the manner of a ſyphon, the ſounds are excited to great advantage, and in this way it was that I diſcovered the low ones.

meatus auditorius may be filled with water: all theſe circumſtances ſhould be occaſionally varied; but above all, the times moſt favourable for exciting them, and a place free from noiſe ſhould be choſen. In ſhort, it will be much better learnt by practice than deſcription; for I imagine that different means are required in different people, becauſe I find a very great diſſimilarity in this reſpect between my right and left ear: in the latter I can much more eaſily excite them than in the former, neither can I always excite uniſon ſounds in both ears by the ſame means. And here it may be proper to remark, that perſons of a light turn of mind, and but ſuperficially acquainted with philoſophical matters, may ridicule theſe kinds of experiments, and laugh at an attempt to deduce a theory of hearing from "a ringing in the ears." But the caſe, I preſume, will be different with thoſe of another caſt, who examine the matter with due attention, and reflect, that the moſt important diſcoveries in philoſophy have been ſuggeſted by the moſt trifling and even childiſh phenomena. For a long time I could excite no ſound in my left ear deeper than what anſwered to the middle D of a German flute. I have ſince gone as low as B; but in my right ear I can now go full two notes lower, viz. to G. As it is with the utmoſt difficulty that theſe low notes can be raiſed, it ſhould ſeem that there are others ſtill deeper, but which are not excitable by the means above deſcribed. Alſo in my left ear, I can raiſe notes from B to about an octave above, in all the intermediate gradations, or ſenſible differences; but from thence, to a great part of another octave, I cannot yet excite them, though ſtill higher they may be raiſed in great plenty, but in a more confuſed manner; and they ſeem alſo as if they were in a different part of the ear, or more inwardly than the lower ones. From theſe conſiderations, it ſeems to appear that there is a regular gradation of them from the loweſt to the higheſt; though, on account (I ſuppoſe) of the particular ſtructure of the ear, and my being able to preſs it only externally, I cannot yet excite them. In the right ear, ſeveral ſounds, intermediate to thoſe juſt mentioned, are diſtinguiſhable; and in both ears I can excite many ſounds which are evidently uniſons to each other. A rumbling noiſe generally heard in making theſe experiments is not to be confounded with the ſounds above deſcribed, but to be conſidered as ariſing from a vibration of the internal air, communicated to it by the motion of the tympanum, as will hereafter be deſcribed.

Now, from the above experiments does it not appear (analogous to what has been ſhewn with regard to the eye) that "there are ſounds liable to be excited in the ear, which do not at all depend on the pulſes of the air?"

If this be granted, may we not extend the analogy farther, and reaſon in the manner following?

It is demonſtrated by philoſophers, that ſounds are cauſed by tremors or vibrations in the air. And therefore, "ſince ſounds may be excited in the ear, which do not at all depend on the pulſes of air, they are cauſed by vibrations liable to be excited in the ear, at the ſame times as thoſe aërial vibrations which cauſe the ſame ſounds. Alſo, as there are many different notes or gradations of theſe internal ſounds, there are as many different vibrations liable to be excited in the ear for cauſing them." The uſes of theſe ſounds may likewiſe be preſumed to be analogous to what was ſhewn of the innate colours; viz. "That the air, external or internal, could not conveniently be made to communicate its vibrations immediately to the auditory nerve, but that the interpoſition of thoſe ſhewn to exiſt in the ear, was neceſſary to that end. That, therefore, there are in the ear different times of vibration liable to be excited, anſwerable to thoſe of the air, for cauſing the ſeveral gradations of ſound *. That any one time of aërial vibration, acting on the ear, excites only that internal one with which it is in uniſon, not at all affecting the others, and therefore cauſes only the anſwerable ſound. And that in a mixture of ſeveral ſorts or times of aërial vibration beating on the ear, each ſort excites only its uniſon vibration, as was ſhewn to be the caſe with regard to colours."

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* The ſcale of audible ſounds is ſaid to be about eight octaves; and the loweſt ſound is cauſed by a vibration at the rate of thirty in a ſecond.

SECTION IV.

Concerning the Manner in which we obtain an Idea of the ſituation of ſounds, and other Phenomena of

HEARING.

MY deſign in this Eſſay was to eſtabliſh the doctrine of Internal Sounds, as delivered in the laſt ſection. With regard to the ſubject of this, I confeſs I have nothing to offer which I can ſatisfy myſelf of the truth of by experiment, and would only wiſh to excite the attention of philoſophers to ſo curious an inquiry.

OBSERVATION I. The ſounds which are excited by preſſing the ear, are weak in compariſon to the loudneſs with which they may be excited by the vibrations of the air; and yet it is well known, that the ſound of a muſical ſtring is much more eaſily excited by ſtriking it, than by an uniſon vibration. Hence we find, that, in the ear, proper methods are employed for increaſing the force of the pulſes of theſe vibrations. I paſs over what ſhare the tympanum, the bones, &c. may have in this intention, and ſhall only obſerve, as others have before me, that the labyrinth and cochlea are contrivances ſimilar in principle to the very means which we uſually employ for the purpoſe of increaſing the loudneſs of ſounds. One deſign of the ſtructure which we obſerve in the ear, therefore, ſeems to be to increaſe the force of the pulſes of aërial vibrations, the better to enable them to excite the internal ſounds.

Obs. II. If the centres of both eyes are preſſed, only one concave appearance is formed; which ariſeth from hence, that every part of the retina of one eye has a correſponding part in the other, as was ſhewn in the firſt ſection. but if the ears be preſſed, the ſounds do not appear to be thus united, but thoſe of each ear are heard as on the reſpective ſide of the head. This difference is the more remarkable, becauſe experience ſhews that we hear ſingly with both ears, even as we ſee ſingly with both eyes.

Obs. III. In exciting the innate colours, a wide circular ſcene appears, in which, as in the retina, objects may be placed in different ſituations with reſpect to each other: but nothing anſwerable to this can be obſerved on exciting the innate ſounds; neither does the cochlea or labyrinth appear to be at all adapted to ſuch a purpoſe.

Obs. IV. Sounds may be excited in the ear by the vibrations of the air though the tympanum and little bones be deſtroyed, as hath been obſerved by others. The cochlea and labyrinth, therefore, are properly the ear, as the retina is properly the eye; and the tympanum, bones, &c. are only appendages ſubſervient to certain conditions of hearing, as the humours of the eye are for ſeeing.

Obs. V. It has been thought by ſome, that aërial ſounds, from whatever quarter they come, affect the ſame parts of the ear, becauſe vibrations in the air are ſuppoſed to be propagated alike on all ſides. We readily judge, however, at firſt hearing, from what quarter without us a ſound comes; and this is ſo true, that (as in optics) a ſound is heard as in that place or from that quarter from whence it was laſt reflected: and when walls, or hills, are duly poſited, the original and the reflected ſounds are both heard as in their reſpective places. So in a concert, we hear many ſounds diſtinctly from each; and not that only, but we judge immediately from what quarters around us the different ſounds heard at the ſame time come. Yet, whoever conſiders the ſtructure of the cochlea and labyrinth, and the general, confuſed, and ſimilar manner in which ſounds muſt enter them, from whatever part they arrive, muſt own, that they do not ſeem at all calculated to anſwer to theſe phenomena. After the moſt attentive conſideration, it ſeems to me that the tone or note, and the ſtrength or loudneſs, are the only parts of hearing with which ſound is concerned.

OBS. VI. I had often wondered why the malleus ſhould be fixed to the tympanum, and that a cord, or nerve ſhould run acroſs that membrane behind, becauſe they ſeemed to me to hinder its vibration. Nature, however, has a reaſon for every thing ſhe does, and therefore ſore ſome purpoſe is undoubtedly anſwered by them. It has ſince occurred to me, that if the tympanum had been free, like the head of a drum, it would, like that, have been capable of yielding only one ſound or note at a time *; and therefore for every different note it muſt have been proportionally tightened or relaxed. But on the contrary, we can hear ſeveral, or many different ſounds at the ſame time, and even judge of their ſituations, as obſerved above; which perhaps could alſo not have obtained if the tympanum had been free.—Are not aërial pulſes, according as they come from a different quarter, made, by means of the external ear and meatus auditorius, to beat upon a different part or ſide of the tympanum?—Do not the contrivances abovementioned prevent the vibration being uniformly communicated to the whole membrane, and confine it to the part immediately affected?—Any part being paſſive to, or liable to receive, any time of vibration that the air may impreſs on it, and dif-

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* Whether this holds with regard to receiving and tranſmitting of ſounds from the air, as well as by beating it with a ſtick may be doubted. It would only perhaps be done leſs clearly or diſtinctly when free, than otherwiſe.

ferent vibrations, in different parts, at the ſame time?

Obs. VII. By repeated experiments, I find that the internal ſounds are not in the tympanum. When I touch that membrane with a probe, I find indeed that it has a motion, and that motion occaſions the rumbling noiſe which ariſeth on exciting the internal ſounds, as before obſerved. But I take it that it occaſions that noiſe only by acting on the internal air, and thereby exciting the anſwerable innate ſound. This motion I imagine is cauſed by an alternate action of the muſcles of the malleus, which pull it to and fro, for it is a motion of the whole membrane: it ſeems to be performed but very few times in the courſe of a ſecond, and is always, as far as I can find, about the ſame ſwiftneſs, though the ſtrength or latitude of the motion is very variable. I ſuſpect that this motion of it is continual in ſome degree; for if a wilk-ſhell, or other hollow body be applied to the ear, a noiſe is heard like the waves of the ſea; which is a very common experiment, and ſeems to proceed from the motion of the air (in conſequence of ſuch action of the tympanum) reflected and made ſenſible by the hollow body ſo applied. I repeat, however, that the internal ſounds are not in the tympanum: neither does it appear to me that the whole tympanum, or the muſcles of the malleus, are by any means capable of alternate action ſo ſwift as the vibration of aërial ſounds, ſome of which are at the rate of ſeveral thouſands in a ſecond. Conſiderations like theſe have led me to ſuſpect that the motion of the whole tympanum, above deſcribed, and of the little bones, have leſs ſhare in the buſineſs of ſound or actual hearing than has been commonly imagined *. Are they not either wholly or chiefly ſubſervient to the purpoſe of exciting the paſſions, and other affections of the mind and body, which we find, by experience, to be the conſequences of certain conditions of aërial ſounds?

Obs. VIII. The tympanum is ſo very ſenſible that we cannot well make experiments on it.

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* It has been thought that the whole tympanum is put into vibration by every different ſound; but this will appear abſurd, when two or more ſounds are acting on it at the ſame time.

it. Yet if any one chooſes to try, he will find, by touching it with a probe, that the ſenſation does not ſeem to be confined to that point, but to affect the ſyſtem in a more general manner. If the motion of the whole tympani of both ears, deſcribed above, be ſenſibly excited, eſpecially if the ears be cloſed, the ſenſation ſeems to fill or ſurround the whole head. I had a patient (to whom I can refer the doubtful) who appeared, by the ſymptoms, to have had a ſuppuration in the barrel of the right ear; for putrid matter was afterwards diſcharged from thence into the mouth. If this patient leaned her head forward, ſhe felt, as ſhe expreſſed herſelf, *her brains fall forward*, and if afterwards ſhe held it backwards, ſhe thought ſhe felt *her brains fall backwards again;* which made her fancy that her brain was looſe in her ſkull; but if ſhe lifted her head up, by placing her fingers about that ear in a particular manner, thoſe things did not happen; and thence ſhe thought her brain, during that time, was reſtored to its right place. From the laſt-mentioned circumſtance, the cauſe of this ſeeming affection of the brain was undoubtedly in the ear; perhaps one of the little bones had been looſened by the ſuppuration, or matter may have floated in the cavity: hence, according as her head was moved forward or backward, it fell againſt the fore or back part of the tympanum, or elſe of the barrel itſelf, but which motion was prevented by placing her fingers and lifting her head in the manner deſcribed. Theſe phenomena ſeem to indicate that the nerves which ſerve either the tympanum or barrel for the ſenſe of feeling, are ſo diſpoſed in the ſenſory or brain, that if the organ be affected in one point, the ſenſation ſhall be felt, not as in the part affected, but as in the fore part of the head. If in another part, it ſhall be felt as in the back part of the head; and perhaps there are other points of that organ which correſpond with the whole ſurface of the head reſpectively. If this be the caſe, then, if the air beats upon a certain point of the tympanum, a ſound ſhall be excited in the labyrinth, and at the ſame time a tremulous ſenſation of feeling, of the ſame degree of ſwiftneſs, ſhall be excited in the fore part of the ſenſory or head, and thereby give us the idea of the ſound coming from the front; for the ſenſe of feeling affects us more powerfully, or is more intimate to us than that of ſound; and therefore the attention of the mind is chiefly engaged by the former, and thence tranſlates, as it were, the ſound itſelf thither; or rather, the ideas are aſſociated by cuſtom or habit. To give an inſtance of this ſuperior power of feeling: though the ſhapes of objects are painted in the retina, yet it is not merely by theſe pictures that we get the idea of the ſhapes of thoſe objects; the eye only conſiders a point of an object at a time; and it is by the motion of the eye itſelf, or of the body, tracing its boundaries, that we get the idea of its ſhape; ſo that it is done by feeling, not by colour merely, as others have already obſerved. Every one who has been weakened by a fever will remember how painfully the ſenſe of feeling is excited by ſtrong ſounds, and the particular manner in which they affect the head. In general, therefore, if an aërial ſound comes from a certain quarter without us, it ſeems to be made, by the contrivance of the external ear, to beat upon a particular part of the tympanum; the ſound, anſwerable to the time of vibration, is excited in the labyrinth; and at the ſame time, a like tremulous ſenſation of feeling as in that part of the head which anſwers to the ſituation of the external ſound, and this, from cuſtom, gives us the idea of the ſound coming from ſuch a quarter. And if two or more ſounds come from different quarters without us at the ſame time, they ſeem to be made, by the funnel of the ear, to beat upon different parts of the tympanum; and beſides the ſounds which they excite in the labyrinth, cauſe reſpectively uniſon or like tremulous ſenſations of feeling, as in the anſwerable ſides of the head; whence we come to underſtand their ſituations externally with reſpect to each other and to ourſelves. The tympanum may move as a whole, notwithſtanding theſe vibrations excited in different parts of it; and (by the mediation of the little bones) *according as its motion as a whole is affected by theſe particular vibrations, the paſſions or affections of the mind and body may be influenced.* I do not know whether the harmony and diſcord of ſounds, conſidered muſically, may not partly depend on this latter principle.

Obs. IX. That it is by the ſenſe of feeling as above, or, at leaſt, that it is not by mere ſound that we get the ideas of the ſituations of external ſounds, ſeems alſo to appear by the following conſiderations. Admitting that different ſides of the tympanum are affected, as above, according as ſounds come from different quarters, yet the nature of aërial vibrations, and the ſituation of the tympanum with reſpect to the paſſage into the labyrinth, ſeem to be ſuch that the ſounds cannot be directed in right lines from the different parts of the tympanum affected, through that paſſage, as the rays of light croſs in the eye and paſs on to the reſpective parts of the retina, for painting objects anſwerable to their ſituations outwardly; and even ſuppoſing they could, yet the ſtructures or ſhapes of the cochlea and labyrinth are ſuch as that no advantage could be derived from this rectilinear direction; they ſeem to enter into the organs in a confuſed medley, through every part of their windings, and alike from whatever quarter they come; and it is only the internal ſounds, depoſited in thoſe organs, that ſeperate them, according as they happen to be in uniſon. Sound, therefore, ſeems to have no other concern in the affair of hearing than merely as to the note or tone, and the ſtrength or loudneſs thereof, as obſerved before.

Obs. X. Diſtinct hearing ſeems to be concerned only with a particular part of the tympanum, as diſtinct viſion is only with the middle of the eye. If we would view an object diſtinctly we turn the optic axis towards it, and even to the ſeveral parts of it ſucceſſively; and if we would hear any ſound to perfection, we turn our face towards the quarter from whence the ſound comes; in ſuch poſition of the face, the ſound falls perhaps on the middle of the tympanum; or at leaſt on that part of it which ſerves for diſtinct hearing. Do the ſounds in this caſe fall upon the malleus?—or rather, do they not fall in the midway between the malleus, and the margin of the membrane? At leaſt, do they not beat upon ſuch a part of the tympanum, as that they may be thrown to moſt advantage into the feneſtra ovalis, or elſe upon the elaſtic membrane in the rotunda?

Obs. XI. Diſtance, with regard to hearing, ſeems to depend on much the ſame principles as diſtance with regard to viſion, viz. on the faintneſs and indiſtinctneſs of the ſound beating on the tympanum, &c.

Obs. XII. Though we have two ears, yet we hear ſingly, even as we ſee ſingly with both eyes. Are the nerves for the ſenſe of feeling, which ſerve the tympanum (or that part of the ear, whatever it be, which gives us the idea of the ſituation of ſounds), joined together in the brain, as the fibres of the retinæ of the anſwerable parts of both eyes are ſuppoſed to be? or is each ear concerned only with the anſwerable ſide of the ſenſory? or do we hear only with one ear at a time, as ſome have ſuppoſed that we ſee only with one eye at a time?

Obs. XIII. May it not be on account of the diſtance of the labyrinth and cochlea from the outward ear, that the innate ſounds are difficultly excitable by external preſſure, and that many of them cannot be excited thereby at all? the deepeſt of theſe ſounds may be in the cochlea, and the higheſt in the labyrinth; for the high and low ſounds ſeem to be in different parts of the ear. As I cannot excite ſounds lower than G, is it not that only the higher ones in the labyrinth, and not the lower ones in the cochlea, are excitable by outward preſſure?

Obs. XIV. Thoſe who are difficult of hearing, and alſo ſuch as liſten attentively, are apt to open their mouths, and then they hear better. It has been ſaid that in this caſe the ſound paſſes by the Euſtachian tubes; but whoever chooſes to make the experiment will find, by putting a finger into each ear, that when he opens his mouth, the bones of the lower jaw leave the meatus auditorius much wider than when the mouth is ſhut, which may perhaps be the cauſe of the phenomenon, at leaſt in part.

Obs. XV. The vibrations in the ear by which the internal ſounds are cauſed, are not of the nature of thoſe of muſical ſtrings, for the following reaſons: the length of the fibres, eſpecially for the low notes, would be too great for them to be contained in the ear, if they cauſed ſounds by vibrations in the manner of muſical ſtrings. If it be objected, that their tenſion is proportionally leſs, I anſwer, that then by reaſon of their ſlackneſs they could not vibrate, on that principle, at all. 2*dly*, There are no fibres in the ear but what are immediately ſurrounded by ſuch ſubſtances as would totally hinder a vibration in the manner of muſical ſtrings. 3*dly*, There is no trace of any contrivance in the ear, which can in the leaſt favour a ſuppoſition of this kind to him who properly conſiders the above objections; and there are many others which may be urged, but theſe are I imagine ſufficient. In what particular manner theſe vibrations are performed I cannot determine: it may be by means of fibres whoſe particles or elements, when irritated, alternately approach towards, and recede from, each other, and thereby lengthen and ſhorten the fibre by turns, without forming the harmonic curve like a muſical ſtring. The fibres for the deeper notes may be compoſed of larger particles, and thence vibrate more ſlowly; and on the contrary, the fibres for the higher notes may be compoſed of ſmaller particles, and thence vibrate more ſwiftly; the pulſes in both caſes being communicated to the nervous fibrils with which they are reſpectively ſerved: the like may be the caſe in the eye, and the ſenſe of feeling. A ſingle ſtring, or even two particles only, ſeem proper for this purpoſe, and they would alſo take up leſs room in the organs; and be too minute perhaps to be diſcovered by the eye. Suppoſing this to be true, we have a reaſon why in the experiment of preſſing the centre of the eye, the green, blue, and violet-making vibrations are moſt eaſily excited, and that their nerves are laſt paralytic; and contrariwiſe with the yellow, orange, and red-making ones. The particles of the former being leſs, are more eaſily put into vibrations, and leſs apt to preſs on the nervous fibrils than thoſe of the latter, which are larger *; and this may alſo be one reaſon why the low innate ſounds cannot

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* By preſſing the centre of the eye for ſome time, a luminous ring of a reddiſh yellow colour is often perceived; if the preſſure be removed, and the eye turned towards the light of the clouds, the ring (which does not diſappear for ſome time) is changed into directly oppoſite colours, viz. green, blue, and violet; which ſhews that the preſſure had rendered the former internal colours more paralytic than the latter, agreeable to what was ſaid before. M. Le Cat, if I remember right, has experiments to the ſame purpoſe, which may therefore be explained by this theory.

be excited by outward preſſure, the particles of the fibres, being large, requiring a ſtronger irritation than can be there applied. But this is conjecture.

Obs. XVI. There are (it may be preſumed) many octaves of the internal ſounds; but not quite one octave of colours. This difference was requiſite, becauſe there may be a great number of vibrations made in the air, which would be loſt to us, if there were not anſwerable ones in the ear; whereas the vibrations of the rays of light being limited to about a ſixth, only the like latitude of internal vibrations was required anſwerable to them.

Obs. XVII. In the eye there ſeem to be a great number of vibrations which give the ſame colour diſperſed in every part of the retina; and vibrations, in all the different times, ſeem likewiſe to be mixed equally together in all parts of that organ. Thus, if any part of the retina be excited by preſſure, not a ſingle colour ariſeth (unleſs by accident, as in the inſtances related in the firſt ſection,) but a white one, compoſed of all the others. But by exciting the innate ſounds I can hear many of them diſtinct; nor can a ſound be excited compoſed of all the internal ones, as is the caſe with colours. The reaſon of this difference is, that the internal colours can be mixed together in the retina, in a ſpace ſmall enough for them to be perceived only as one colour. But this cannot take place with regard to the internal ſounds, by reaſon of their far greater number.

OBS. XVIII. About ten years ago, I obſerved that a flute, an hautboy, a trumpet, and other inſtruments, though they were made to yield ſounds which were in uniſon with each other, and equally loud, yet had a difference which every one could obſerve, and which I then called the *mode* of ſound. Thus alſo the voices of people, and the ſounds yielded by various bodies, though of exactly the ſame tone and ſtrength, had a ſimilar difference. Whether the cauſe of this curious phenomenon had been diſcovered, I could not learn; but by meditating on the ſubject, and making ſeveral experiments, I found that theſe ſounds were not ſimple, but compoſed of others, of which theſe were only the reſult or aggregate, even as the colours of bodies are various compounds of the ſeveral original colours. I am told, by a gentleman to whom I communicated this theory, that a diſcovery of this kind is already made public, though I have not yet been able to get a ſight of it: I ſhall not give the obſervations which ſuggeſted that theory to me, leſt they ſhould be ſimilar to thoſe alluded to *. But as I cannot find that an explanation has been given

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* The principle on which theſe ſounds were capable of being decompoſed was, that in many caſes ſome of the ſounds in the mixture were ſtronger, and others weaker: hence if they were excited as gently as poſſible, or rather if I removed the cauſe of them to a ſufficient diſtance from me, the weak ſounds in the compound would be inaudible, and only the ſtronger ones heard. Suppoſe a ſound compoſed of C D and E, that C was double the loudneſs of D, and D double that of E; the tone of that ſound would be in a compound ratio of the tones and ſtrengths of the ingredients; that is, it would be a ſharp C. If E be rendered inaudible, the ſound, as being compoſed only of C and D, would be nearer to C; but if D alſo be made inaudible, the ſound would be pure C. If the ingredients are equal in ſtrength or loudneſs, this decompoſition cannot be made. This theory was ſuggeſted to me by like obſervations with regard to colours; for ſome objects, according as they are more or leſs ſtrongly illuminated, appear differently coloured, for reaſons (as I imagined) ſimilar to thoſe given above.

given of this phenomenon with regard to what happens in the ear, I ſhall here ſubjoin a conjecture concerning it: it was ſhewn above, that all the internal colours may be excited within a ſpace of the retina ſmall enough for them to be perceived only as one colour, the aggregate of the whole. If aërial ſounds are blended together, as in the caſes juſt mentioned, they are excited ſo near to each other in the tympanum, that they are heard only as one ſound, as happens with a mixture of colours; but if the ſounds are excited at a diſtance from each other in the air and tympanum, they are heard diſtinct, as happens when, in viſion, colours are painted in different parts of the retina. But to enter a little cloſer into this reaſoning, thoſe who are verſed in optics know, that if any two neighbouring colours in the refracted ſpectrum be mixed together, the colour ariſing therefrom will be ſuch an one as would be cauſed by the rays in the mean betwixt them: thus, if blue and yellow be mixed together in equal quantities, the colour will be green; and if the quantities be unequal, the green will be tinctured with yellow or blue in proportion; the like may be obſerved of other neighbouring colours. But if red and violet be mixed together, the colour will not be a green, or the intermediate one, but a kind of purple, unlike to any of the original colours. Alſo, if any number of colours are mixed together, provided the two extreme ones are at a ſufficient diſtance from each other in the ſpectrum, there will not be produced the intermediate priſmatic colour, but ſome one unlike to any of theſe: thus, a mixture of all the rays compoſe a white, and ſo of other mixtures; for further information in which, Sir Iſaac Newton's Optics may be conſulted. Now, I would ſuppoſe that a ſingle ſeries of colorific vibrations in the retina are diſpoſed in a right line according to their times, as in a refracted beam of light, and that this line exceeds the diametre of a viſible point, yet is not ſo long as that the two ends of it may be perceived diſtinct. Hence the red and violet only, though they are not ſeparately diſtinguiſhable, yet as they do not fall within a viſible point, they alſo cannot be perceived as a perfect mixture or under the form of the intermediate colour; they muſt therefore be perceived as in a ſtate between perfect mixture, and diſtinctneſs; and we find that a purple is the reſult, a colour in which the ingredients can in ſome meaſure be inferred by the eye. But two colours which are near each other, are contained within a viſible point, and therefore may be ſaid to be mixed intimately together, for they exhibit the proper intermediate colour, as was ſhewn above. For the ſame reaſons, all the colours in the ſeries together ought not to exhibit, like yellow and blue, the intermediate colour, nor any of the original ones, becauſe of the red, violet, &c. which are exterior to the viſible point; neither ought the colour exhibited to be ſuch an one as that the ingredients may in a manner be inferred by the eye, as is the caſe with red and violet alone, becauſe the whole ſeries is a compoſition of perfect and imperfect mixture; and we find that they compoſe a white *. Now if we apply theſe principles

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* If we ſuppoſe a number of theſe ſeries joined together in a right line by their anſwerable ends, viz. red to red, and violet to violet, that the whole ſurface of the retina is filled with ſuch lines drawn parallel to each other, and that theſe lines are croſſed, at the red and violet points, at right angles by ſimilar ones reſpectively; the whole ſurface will be divided

ciples to the ear, and conſider hearing as compoſed of ſound, and a tremulous ſenſe of feeling, as mentioned before, we may be enabled to form ſome idea of the cauſe of the above-remarked difference of ſounds, whoſe tones and ſtrengths are the ſame. The innate ſounds are not, like colours, comprized within ſo ſmall a portion of the ear as that they may be all heard as one: on the contrary, experiment ſeems to ſhew that they are *diſtinct*. The above doctrine, therefore, is not applicable to the innate ſounds; it muſt of courſe be applied to that part of hearing which depends on the tremulous ſenſe of *feeling*, and by which the other is governed, as hath been ſhewn. If two or more aërial vibrations fall within the ſame point of the tympanum, they may be conſidered as mixed perfectly together *; and therefore a ſound

ded into ſquares, the ſides of which will be leſs than the diſtances required for diſtinct viſion.——But this is a mere hypotheſis.

* Query. Whether in any inſtance of this kind, two or more vibrations are converted, as it were, into one?—For example, whether a ſound and its octave make, not the intermediate ſound, but the octave? I do not think this ever to be the caſe; but that they continue diſtinct, and therefore the

a ſound will be cauſed whoſe note is the mean of all theſe, and whoſe mode is the ſame with that which would be produced by a ſingle vibration. But ſounds in the tympanum further apart, yet not ſo diſtant as to be heard diſtinct, though they yield a ſound of the intermediate tone, yet the mode thereof, by reaſon of the imperfect mixture, ſhall be different. And if the ſounds are excited at a ſtill greater diſtance, they ſhall be heard diſtinct; and therefore by aſſuming the hypotheſis of the tremulous ſenſe of feeling, and carrying with us the idea of perfect mixture, of indiſtinction or imperfect mixture, and of diſtinct ſounds, as above, duly combining theſe, varying their ſtrengths, and taking into conſideration what happens in the cochlea and labyrinth with regard to the innate ſounds, and alſo the paſſions or affections of the mind, we may have perhaps a theory of this kind of phenomena. But as this is a ſubject which does not eaſily admit of experimental proof I ſhall not enlarge on it.

the intermediate tone is the reſult. The reaſon why ſome modes are more pleaſing than others, may perhaps be collected from the eighth Obſervation.

SECTION V.

Being an Appendix to the foregoing Eſſay.

IT may not be improper to acquaint the Reader, that my ſituation in life has hitherto been ſuch as to have afforded me but few opportunities of reading books which were not in the line of my profeſſion; many diſcoveries in philoſophy therefore had been made public which I was unacquainted with: many I ſuppoſe ſtill remain, of which I have no idea. As I am fond of amuſing myſelf at my leiſure with ſtudies of a philoſophical nature; ideas have occurred to me which I thought were new, but which I have afterwards found in authors. The doctrine of the modes of ſound occurred to me above ten years ago, (as indeed did almoſt the whole of the preceding eſſay). I mentioned that I had ſince ſhewn it to a friend, who informed me that the diſcovery had already been publiſhed by Tartini, an

Italian. But ſince that eſſay was ſent to the preſs, I happened to meet with a tranſlation of Rouſſeau's Muſical Dictionary, and find by it that the theory in queſtion is not yet known, Tartini's diſcovery being of another kind, viz. *The harmonic ſounds which ariſe in conſequence of any muſical ſound;* and on which he has founded a new ſyſtem of muſic.

M. Rousseau, after explaining the two differences of ſounds, the *tone* or *note*, and the *ſtrength* or *loudneſs*, ſpeaks of this third difference of ſounds; and expreſſeth himſelf as follows:

"In regard to the difference which is found alſo between the ſounds by the quality of the modification, it is evident that it holds neither to the degree of elevation, nor even to that of the force. It will be in vain for an hautboy to place itſelf in uniſon with a flute: it will be in vain to ſweeten the ſound to the ſame degree; the ſound of the flute will always have a *Je ne ſais quoi* of mellow and ſweet; that of the hautboy ſomewhat rude and ſharp, which will prevent the ear from confounding them, without mentioning the diverſity of the modification of the voice.

"THERE is not an inſtrument which has not its particular tone which has no connection with that of another; and the organ alone has twenty methods of playing, all of a different modification. *No one however that I know of has examined the ſound in this particular*, which, as well as the reſt, will perhaps be found to have ſome difficulties; for the quality of the modification cannot depend either from the number of vibrations which forms the degree from flat to ſharp, or from the greatneſs or force of theſe ſame vibrations which forms the degree from ſtrong to weak: we muſt then find in the ſonorous body, a third different cauſe from theſe two, to explain this third quality of ſound and its differences, which perhaps is not too eaſy." Thus far Mr. Rouſſeau.

I SHALL therefore give the Reader the obſervations from which I was afterwards led to ſuſpect this theory, and in the very words of the original paper which I have long had by me.

"——————The innate ſounds are the moſt ſimple poſſible, as they are made (we may ſuppoſe) only by ſingle or ſimple vibrations; whereas the aërial ſounds which we hear, are all of them more or leſs compounded. And on this compoſition of them, the ſolution of ſome curious phenomena ſeem to depend, which have not been attended to by the learned.

"For beſides that ſounds are higher or lower in tone according to the ſwiftneſs of the vibrations which cauſe them, there is alſo another property in which ſounds differ from one another, though of the ſame ſtrength and tone: an organ, for example, has ſeveral kinds of ſtops; there is one which being played reſembles a flute; another which gives the ſame muſical notes as the above, yet differ very much in ſound: for whereas thoſe reſembled a flute, theſe ſound like a trumpet; another ſtop gives the ſame muſical notes as both theſe, yet reſemble an hautboy. The like may be inſtanced in bells, the ſtring of an harpſichord, violin, &c. So I can with my mouth ſound a row of notes, or ſing a ſong in a manner reſembling a flute; I can ſound the ſame notes like a trumpet, an hautboy, and other inſtruments: ſo in converſation, I can talk in a great many different voices, though with the ſame tones and loudneſs. There are hardly two people in the world whoſe voices are exactly alike, though they were to talk in the ſame muſical tones, and equally loud; and it is from hence that we know the voices of people who are talking to us. The ſounds alſo which are yielded by bodies that are ſtruck are different, though of the ſame tones and ſtrength, inſomuch that there are hardly two kinds of bodies which ſound exactly alike. This difference of ſounds I call the *mode* of ſound to diſtinguiſh it from the *tone* and *ſtrength*.

"OBSERVATION I. If I ſtand by a large church bell when it is ſtruck, and liſten attentively to the ſound when it is almoſt vaniſhed, I can diſtinguiſh not one ſound or note only, but ſeveral; for the real note of the bell will go off and be no longer heard, and inſtead thereof other ſounds different in *tone*, as well in *mode*, will ariſe, which in ſome bells are more and in others leſs in number: and different bells exhibit this phenomenon with different degrees of diſtinctneſs.

"Obs. II. I lived near a church in which were eight bells, and the clock ſtruck on the the firſt four of them every quarter of an hour, I have frequently obſerved that if I was ſo ſituated as that the ſounds could hardly be heard, or heard indiſtinctly, the ſounds of theſe bells, which otherwiſe were D, C ſharp, B, A, in theſe caſes totally loſt their ſucceſſion; they gave quite different tones, as G, E, C, B, or others, and their *modes* likewiſe were different.

"Obs. III. I have obſerved the like in the voices of people under ſimilar circumſtances, and alſo in the ſounds produced by various other methods. I mean in caſes where the ſounds were hardly ſenſible, or indiſtinctly heard.

"Obs. IV. I once ſaw a peal of ſmall houſe bells, with which a young gentleman uſed to amuſe himſelf; if I ſtood near theſe bells when he rang them they were very tunable, and made good muſic; but if I removed to a diſtance from them, though I heard the bells diſtinctly ſtruck, they no longer yielded ſounds in ſucceſſion, every one a note lower than the preceding, but quite irregular and confuſed: the irregularity was different at different diſtances, and the *modes* were altered as well as the tones.

"FROM theſe obſervations I gather *that the tones of ſounds which are yielded by bells, &c. are not ſimple, but compoſed of other tones.* Thus in optics, the rays of light in the fourth ſeries of a ſpectrum cauſe a green colour, which being produced by one ſort of rays only, may be termed ſimple; the rays of the third ſeries are yellow, and of the fifth blue; yet when mixed equally together, they no longer appear as yellow and blue; but a colour reſults from their mixture, which is the ſame as would be cauſed by the ſimple rays of the fourth ſeries. If theſe rays are again ſeparated, they no longer cauſe a green, but their proper colours; if the rays are mixed in unequal quantities they cauſe a green, not like the other, but inclining to the colour of the greateſt quantity of rays. Alſo, if either in equal or unequal quantities they are mixed not perfectly together (ſo that for example, if the object from which they come be viewed near, or but faintly illuminated, theſe colours may be ſeen diſtinct; yet if viewed at a diſtance, or the object be ſtrongly illuminated) they cauſe an imperfect green, ſo all the ſeven original colours mixed together produce a white, and the like.

"Bodies in general do not appear exactly of the ſame colours when viewed by a ſtrong, as when viewed by a faint light, which I take to ariſe from hence; that bodies do not reflect an equal quantity of each ſort of rays. Suppoſe that a body reflected four parts of red, three of orange, two of green, and one blue; if the light be ſo faint as that the red rays chiefly be ſenſible, the object will be redder; and if the light be ſtill ſtronger, the colour will vary from that red with the increaſe of the illumination, till it appears of that colour which ought to reſult from the above mixture.

"For the ſame reaſons, if two ſounds are mixed equally together, and of equal ſtrengths, but of different tones, they will cauſe a ſound whoſe tone is the mean between the tones of the two ſounds when ſeparate: thus G and B being mixed, the note will be A; if they are unequal in ſtrength, the tone will incline towards G, or B in proportion; and all the other inſtances of colours above given may be applied in ſome meaſure to ſounds.

"So then the reaſon of the change of tones of ſound by diſtance, inattention, or the like, ariſeth from hence; that if the ſounds which are blended together be of unequal ſtrengths, thoſe which are ſtrongeſt muſt reach to a greater diſtance than thoſe which are weaker; ſo that the weaker ones not affecting the ear, or not with ſufficient force, only the ſtronger ſound or ſounds which reach the ear will be perceived; but if the ſounds are equally ſtrong, this will not take place. The ſounds of the bell in the firſt obſervation, however, are to be underſtood by what was ſaid above of colours being mixed imperfectly together, and therefore they were not heard as one ſound but when they were ſufficiently ſtrong, ſo as to ſpread their effects on the ſenſe into one another *.

"It

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* The note of latitude, as it is called, in wind inſtruments, may depend on the principle of the mixture of ſounds of different

"It has uſually been thought that all ſounds affect the ſame parts of the ear; but the fact appears to be otherwiſe. And it is ſurprizing how near ſounds may be to each other in the air, and yet be heard diſtinct, and even when no longer heard diſtinct, they are preſerved ſeparate: it does not appear that two ſounds form only one vibration when thus mixed, but the intermediate tone. A theory of aërial ſounds in this view therefore is yet wanting, as well as that of different ſounds yielded at the ſame time by the ſame body.

"If the ſounds thus mixed together are concords, they form perhaps the ſweeteſt modes; and on the contrary, the diſagreeable modes ſeem to be compoſed of diſcords. The laſt five or ſix of Bow bells are, I think, the moſt agreeable in peal of any that I ever heard; and the reaſon is that the founder has judiciouſly varied the *modes* as well as the *tones*. This obſervation might probably be applied to good uſe by lovers of muſic.

Now theſe complex ſounds, though we per-

ferent ſtrengths, all of which are not audible but by a ſtrong blaſt.

ceive them as one, do not, I take it, excite that innate ſound in the ear which anſwers to the tone we hear; but every one of the vibrations which compoſe that complex ſound excites its uniſon, which are ſo mixed together in the ſenſe that we perceive only one ſound, the reſult of the whole, as before obſerved.——"

WHAT relates to the ſenſe of feeling (of which I had no idea at the time of writing the above) has already been explained, and which ſeems to have a great ſhare in theſe modes, inſomuch that the mixture of the ſounds, and their being heard as one, depends on it, and not on the innate ſounds, as hath been ſhewn. The innate ſounds may be excited in greater or leſs number; ſtronger or weaker; and in more or leſs harmonic or diſcordant relation to each other, which ſeems to be all that ſound contributes to theſe modes. The tremulous ſenſe of feeling excited in the tympanum, the parts connected with the little bones, the portio dura, the nerve that adheres to the tympanum, the various diſtributions of the twigs of the nerves of the ear, ſo as to form ſympathies with other parts, and the aſſociation of ideas are, perhaps, all concerned in theſe modes, though chiefly the firſt. I have only begun the ſubject, and would wiſh to ſee it further proſecuted by thoſe who have leiſure and inclination. In the mean time it may, I think, be admitted, that "as the colours of bodies are not ſimple, but made up of others, according to the different mixtures of the rays of light iſſuing from them, ſo neither are their ſounds ſimple, but compoſed of ſeveral or many others, which the body by its various vibration emits; and which, like the colours, are ſo mixed together in the ſenſe, as to appear but one, the mean of all the ingredients. The *modes* of the ſounds depend on the manner of this mixture."

A
TREATISE
ON
HARMONIC SOUNDS.

INTRODUCTION.

AS I have had occaſion to mention Muſic in ſeveral places of the preceding Eſſay, the following may be inſerted with ſome degree of propriety.

SOME time ago I found out the theory of the harmonic ſounds yielded by muſical ſtrings, and conſtructed a ſcale of them for four ſtringed inſtruments tuned fifths, without then knowing that they had been publiſhed long before. I have ſince been undeceived by the friend who informed me that Tartini had diſcovered the theory of the modes, and by Mr. Rouſſeau's Dictionary. There are two very material points, however, with reſpect to the practice of theſe ſounds, which I cannot find any account of in that Dictionary; and therefore, as my paper on this ſubject is ſhort, I will ſubjoin the whole of it.

THE

THE

THEORY

OF

HARMONIC SOUNDS.

THE *common* ſound of a muſical ſtring is cauſed by a ſimple or ſingle vibration thereof; the *harmonic* ſound by a various vibration.

IF one of the ſtrings of a violin be ſtruck with the bow open, it vibrates, and thereby yields a ſound: if the finger preſs the ſtring in the middle upon the finger board, its lower part only will vibrate, and its vibration will be twice as ſwift as that of the whole ſtring, ſo that the ſound will be an octave above the former.

BUT if the finger be laid lightly on the ſtring, without preſſing it on the finger board, both halves of it will vibrate; their vibrations will coincide with each other; the ſound ariſing therefrom will be much ſweeter than in the other caſe, and will be the harmonic octave to the open ſtring

If the finger be placed at one third of the ſtring from the nut, and ſtruck with the bow, it will vibrate in three diſtinct and equal parts, coincident with each other. You may prove this by laying another finger lightly at two thirds; for though the ſtring be thus ſtopt double, the ſound will be the ſame; though if you remove either or both of the fingers from theſe points, either higher or lower on the ſtring, the note ceaſeth. Alſo, if with a finger at $\frac{1}{3}$, you bow at $\frac{2}{3}$, no ſuch ſound will be excited; but if you remove the bow ſufficiently from this point either way, the ſound again ariſeth *. The vibration in this caſe being to that

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* You may ſee this threefold vibration, at leaſt in the ſilver ſtring: but in a baſs viol you may ſee it much plainer, and there the four, five, and ſixfold vibrations, may alſo be diſtinguiſhed by the eye. Each harmonic ſtop is not confined to the point, but has a latitude, and the points of the $\frac{1}{2}$ and $\frac{1}{3}$ diviſions have a greater latitude than the leſſer diviſions. The reaſon

that made by ſtopping at one half, as 3 to 2 makes a fifth above it.

PLACE the finger at one fourth of the ſtring from the nut, it will vibrate in four diſtinct and equal portions, coincident with each other, and being twice as ſwift as the firſt, the ſound will be an octave above it.

REMOVE the finger ſucceſſively to $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{7}$, $\frac{1}{8}$, $\frac{1}{9}$, of the ſtring, it will vibrate in 5, 6, 7, 8, and 9 equal parts; and you will, for reaſons ſimilar to thoſe given above, have a ſharp third, a fifth, and very ſharp ſixth (or flat ſeventh) to the octave; an octave to that, and a full tone above that double octave, and you may go ſtill higher, by taking the $\frac{1}{10}$, $\frac{1}{11}$, $\frac{1}{12}$, &c. of the ſtring.

THE above notes may be made by ſtopping at any other point beſides $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, * &c. and like-

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reaſon of this latitude is, that near the points the motion of the ſtring is very ſmall, and therefore is not much interrupted by the finger; whereas, nearer the middle, the vibration is more eaſily ſtopt. The tone is alſo a little altered when the finger is not exactly on the point; the reaſon is obvious.

* Any of theſe frets may be uſed in practice, when more convenient than the other.

wiſe by ſtopping at all, or more than one of theſe diviſions: thus, the fifth above the key may be made by ſtopping at $\frac{2}{3}$ from the nut, as well as at $\frac{1}{3}$, and alſo by ſtopping at both $\frac{1}{3}$ and $\frac{2}{3}$. The octave may be made by ſtopping at $\frac{1}{2}$ or $\frac{3}{4}$ as well as at $\frac{1}{4}$ from the nut; and alſo by ſtopping at all, or more than one of theſe points (which may be done by threads faſtened acroſs the ſtring round the inſtrument). The ſharp third above the octave may be made by ſtopping at $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, as well as at $\frac{1}{5}$, and likewiſe by ſtopping at all, or more than one of theſe points; and ſo of the reſt. Whence you have this caution; "that if you do not happen to ſtop right, ſome other note than that intended may ariſe." Thus, if you place your finger a little below $\frac{1}{3}$ from the nut, you light on one of the $\frac{1}{5}$ diviſions, and ſo have, inſtead of a fifth, the ſharp tenth: a little lower you fall on one of the $\frac{1}{7}$ diviſions, which gives the ſharp 13 (or flat 14) and ſo of others; of which therefore you muſt be aware.

ALSO you muſt be careful not to bow upon the points or diviſions of the ſtrings, for then either no ſound will ariſe, or not that deſigned, but between theſe points: thus, if you ſtop at $\frac{1}{4}$ you muſt not bow at $\frac{3}{4}$, but between that and the bridge, or between other points, though that next the bridge is beſt, the ſtring being moſt ſteady there. From whence alſo it appears "that you muſt bow nearer to the bridge in proportion as you uſe an higher fret," the diviſion being leſs.

LIKEWISE when you ſtop at $\frac{1}{2}$, you will, inſtead of the key, often get its octave, unleſs you bow towards the verge of $\frac{3}{4}$ from the nut, becauſe the $\frac{1}{2}$ fret is alſo a $\frac{1}{4}$ diviſion *. But by bowing near to $\frac{3}{4}$ you do not excite the octave, for reaſons which may be ſeen above; and ſo of other notes.

A STRING is ſo apt to run into harmonic vibrations, that theſe ſounds may be raiſed merely by bowing on proper parts of it, without ſtopping with the finger: thus, if you bow on the proper parts of the ſilver ſtring near the bridge, you have thirds, fifths, eighths,

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* It is likewiſe a $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{10}$, &c. diviſion. The like may be obſerved of the $\frac{1}{3}$, and other frets: and the various reſpective ſounds may be raiſed by bowing properly, as above.

and other harmonic notes ; and they may likewiſe be raiſed by bowing on other parts of the ſtring, by obſerving what was ſaid in the preceding paragraph. The bow, in theſe caſes, acts in a double capacity, for it both ſtops and vibrates the ſtring *.

FROM what has been ſaid, it appears " that the harmonic ſounds are made by ſtopping the ſtring lightly, according to the proportions in

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* I find by the Dictionary of Muſic, that Tartini has founded his ſyſtem on this obſervation : I am miſtaken, however, if he has not proceeded on a wrong principle. He ſays (if I remember right, for I have not the book now by me), that when a ſtring is ſounded, all the notes harmonic to the ſound naturally ariſe with it ; and he applies it to all other ſounds It is true that if you ſtrike a ſtring with a bow, you will often raiſe ſome of the harmonic ſounds, for a reaſon given in the laſt paragraph ; and that a ſtring ſhould vibrate as a whole, and in diſtinct parts at the ſame time, is as eaſy to conceive as that the moon can revolve at once round the ſun and our earth. If, however, you excite the ſound of a ſtring by any other means, as by ſtriking it with a ſtick, or pulling it with the finger, I do not find that any ſuch ſounds ariſe. I have not yet had leiſure to ſatisfy myſelf concerning this matter ; but mean to examine both this, and what Tartini ſays of the *third ſounds*, when I have a convenient opportunity.

the following ſeries, viz. $\frac{1}{2}, \frac{1}{3}, \frac{1}{4}, \frac{1}{5}, \frac{1}{6}, \frac{1}{7}, \frac{1}{8}, \frac{1}{9}$, &c. the ſtring vibrating in as many diſtinct and equal portions as the denominator hath units, all in uniſon with each other; and the ſounds being higher according as the portions of the ſtring become ſhorter; that is, according to the ſwiftneſs with which thoſe parts vibrate." This may ſuffice for the theory of theſe ſounds, we may now proceed to

THE PRACTICE.

The ſtrings of the violin, &c. being tuned fifths *, the harmonic notes on them will be as in the following ſcale.

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* By means of theſe ſounds the inſtruments may be tuned to the greateſt exactneſs: to do which you have only to ſcrew up the ſtrings ſo as to bring the 5, 9, 13 on the line $\frac{1}{2}$ in uniſon with the 5, 9, 13 in the line $\frac{1}{3}$ (ſee the ſcale); and as the ear can better judge of an uniſon than a fifth, you may tune to greater perfection than in the common way. This alſo I could not find in Mr. Rouſſeau's Dictionary.

SCALE.

Fourth String	1	5	8	$10^{\text{Sh.}}$	12	$14^{\text{Fl.}}$	15	&c.
Third String	5	9	12	$14^{\text{Sh.}}$	16	$18^{\text{Fl.}}$	19	&c.
Second String	9	13	16	18	20	$22^{\text{Fl.}}$	23	&c.
Firſt String	13	17	20	22	24	$26^{\text{Fl.}}$	27	&c.
The diviſions from the nut	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{5}$	$\frac{1}{6}$	$\frac{1}{7}$	$\frac{1}{8}$	&c.

You may carry it ſtill higher, by adding frets above $\frac{1}{8}$; but this commands a ſufficient compaſs of notes for practice. It has, however, the inconvenience of being incomplete, eſpecially in the loweſt and beſt notes.

With a view therefore to improve this ſcale, or obtain one more perfect, imagine the ſtrings to become continually ſhorter, or that the bridge and nut approach toward each other with a regular motion; the diviſions of $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, &c. will ſtill remain in the ſame proportions, but on a ſcale continually contracting or leſſening; and the ſounds will become higher in proportion to the ſhortneſs of the ſtrings.

To reduce this to practice, place the little finger lighty on $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, or any other harmonic fret of either of the ſtrings; place the fore finger on the nut, and ſtrike the harmonic ſound with the bow, continue the bowing while you ſlide your fore finger from off the nut upon the ſtring, preſſing it down hard on the finger-board, and from thence along the ſtring up towards the bridge, bringing your little finger nearer towards it, ſo as that you may be always at $\frac{1}{3}$ (if you uſe that fret) of that part of the ſtring between the fore finger and bridge, ſo ſhall you have a continually aſcending harmonic ſound. From whence it appears, “ that you may make an harmonic ſound of what degree of ſharpneſs or flatneſs you pleaſe” (within the compaſs of the inſtrument) : with ſounds made after this manner, therefore, you may ſupply the deficiencies of the above ſcale, at leaſt from 5, upwards; whereby you may make it as perfect as you pleaſe.

Or you may compoſe your ſcale intirely on this plan, (though it muſt be owned that the notes are leſs harmonious than when the ſtrings are not hard ſtopt) ; thus, make G, A, B, C, on the ſilver ſtring as in the common way, ſtopping hard for thoſe notes with the fore finger, and making them harmonic by ſtopping lightly with the little finger at $\frac{1}{3}$ of that part of the ſtring between the fore finger and bridge, and ſo on with the other ſtrings. By this means you have a compaſs of ſixteen notes, uſing only one fret, and going no higher than A on the treble ſtring. But by ſhifting the hard ſtop to B, C, D, &c. you may go ſtill higher; and higher after all by changing the fret for thoſe above. You may alſo ſhift on the other ſtrings, and on any part of any ſtring you may by this means make not one only, but as many harmonic ſounds as your fingers can command frets. The practice indeed is ſomewhat difficult, but can be done, I imagine, ſufficiently well by one uſed to ſhifting and double ſtops; or, in other words, by a maſter of the violin.

BUT the beſt ſcale for practice that has yet occurred to me is the following.

A Scale of the harmonic notes of the violin, according to the Diatonic Genus; which therefore might eaſily be varied for the other genera, and alſo for other inſtruments of the the viol kind.

Take only the following notes of the former Scale.

G	D	A	E	$\frac{1}{8}$
*	*	*	*	$\frac{1}{7}$
*	*	*	B	$\frac{1}{6}$
B	F^{Sh.}	C^{Sh.}	G^{Sh.}	$\frac{1}{5}$
G	D	A	E	$\frac{1}{4}$
D	A	E	B	$\frac{1}{3}$
G	*	*	*	$\frac{1}{2}$

Theſe notes run thus: G, *, *, *, D, *, *, G, A, B, *, D, E, F ſharp, G, A, B, C ſharp, D, E, *, G, A, B, *, *, E.

I have rejected the D, A, E in the line $\frac{1}{2}$, and alſo thoſe in the line $\frac{1}{6}$, becauſe they are more convenient for playing in the lines $\frac{1}{3}$ and $\frac{1}{4}$: I reject all the notes above $\frac{1}{8}$, and likewiſe thoſe in $\frac{1}{7}$, becauſe they are too difficult to hit, and becauſe thoſe made by the hard ſtop are more harmonious. Perhaps thoſe on $\frac{1}{8}$ might alſo be rejected for the ſame reaſon. The vacancies may be filled up as follows.

The fingers not being long enough to complete the notes from G to D, the ſcale can only be perfected from D upwards.

In order to this, place your fore finger on the ſilver ſtring, as for making the common G ſharp, preſſing the ſtring down on the finger-board, as in the common way, at the ſame time lay your little finger as lightly as poſſible on the ſtring at one third part of the diſtance between your fore finger and the bridge, ſound with the bow, and you have the harmonic D ſharp.

Remove the fore finger, as for making the common A, and the little finger a little farther on, you have E.

Make common A ſharp with your fore finger, and remove your little finger ſomewhat nearer to the bridge, you have F.

MAKE the common B with your fore finger, and remove your little finger a little farther on, you have F ſharp.

MAKE the common ſharp C with your fore finger, and place your little finger at one third part of the diſtance between your fore finger and the bridge, you have G ſharp.

A ſharp, C, C ſharp, D ſharp may be made on the third ſtring in the ſame manner as D ſharp, F, F ſharp, G ſharp were made upon the fourth; and in a ſimilar manner you may proceed to fill up the vacancies in the remaining part of the ſcale, the particular directions for which would be needleſs, after having explained ſo fully thus far. *The $\frac{1}{3}$ ſtop always making the harmonic octave fifth to the note made by the fore finger in the common way.*

IT is ſomewhat difficult for thoſe whoſe fingers are ſhort, to command the $\frac{1}{3}$ ſtop to advantage. In that caſe, the $\frac{1}{4}$ ſtop may be uſed as follows; but the ſcale can only be completed from the ſecond G upwards.

MAKE common G ſharp on the ſilver ſtring with your fore finger, and at the ſame time lay your little finger as lightly as poſſible on the the ſtring at $\frac{1}{4}$ of the diſtance between your fore finger and the bridge, you have the harmonic G ſharp.

MAKE the common A ſharp, C, and C ſharp with your fore finger, and place your little finger at one fourth of the diſtance between your fore finger and the bridge as lightly as you can, you have the harmonic A ſharp, C, and C ſharp. And in the ſame manner you may proceed with the other ſtrings. For obſerve, that *whatever note you make with your fore finger in the common way, by laying a finger lightly on the ſtring at one fourth of the diſtance between your fore finger and the bridge, you make the harmonic double octave to that note*: which rule is perfectly plain and eaſy for practice. The ſounds, however, are not quite ſo fine as thoſe made by ſtopping at $\frac{1}{3}$; and in neither caſe are they ſo fine as when made by the open ſtring, without the uſe of the fore finger, except in the inſtances mentioned before; for which reaſon the notes in the ſcale above ſhould be uſed whenever they can.

N.B. If the ſtring be preſſed down, not with the fleſhy part of the fore finger, but with the nail, the ſounds will be much better. Alſo *in general* if, when you have ſtruck a note, the finger which makes the light ſtop be taken off from the ſtring together with the bow, the ſound will continue a while after, and therefore be more pleaſing; in the open ſtring eſpecially this has a fine effect when properly executed. But if this rule be obſerved only when you uſe the hard ſtop, and not when you uſe the open ſtring, the ſounds will be brought to an equality of ſweetneſs; at leaſt a good performer will be able to do it ſo well, that the difference ſhall not be ſenſible to an ordinary ear. The rule, however, may be obſerved to great advantage in the following ſcheme, where only open ſtrings are uſed. (*That ſcheme, and the perfecting of the former one by means of the hard or fore finger ſtop, are the two particulars which I could not find in Rouſſeau, as mentioned before.*)

A **Deſcription** of an **Harmonic Viol**; the Scheme of which may be applied to any inſtrument of the viol kind.

THE SCHEME.

Fourth String	1	5	8	$10^{\text{Sh.}}$	12	$14^{\text{Fl.}}$	15	&c.
Third String	2	6	9	$11^{\text{Sh.}}$	13	$15^{\text{Fl.}}$	16	&c.
Second String	3	7	10	$12^{\text{Sh.}}$	14	$16^{\text{Fl.}}$	17	&c.
Firſt String	4	8	11	$13^{\text{Sh.}}$	15	$17^{\text{Fl.}}$	18	&c.
Diviſions from the nut	$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$	$\frac{1}{5}$	$\frac{1}{6}$	$\frac{1}{7}$	$\frac{1}{8}$	&c.

When you are playing the violin, or other viol in the common way, and would introduce at times the harmonic notes, you muſt do it according to the directions already given. But for playing a piece all through in harmonics, you may uſe the above ſcheme, *the ſtrings of the inſtrument being tuned each one note above another*. The notes will then lie in a very natural and eaſy order for playing; and the ſtrings being open, you may manage theſe ſounds to the greateſt advantage. You may tune it to any inſtrument or pitch at pleaſure; and you may alſo flatten or ſharpen any of the ſtrings anſwerable to the key, only remembering that all the notes on theſe ſtrings are then flat or ſharp; and as moſt of the notes are double, you cannot be at a loſs for the natural ones, &c. Imagination alſo may make this ſcheme ſtill more complete: if, for example, you pitch in 8, you may ſharpen the ſecond ſtring, if a ſharp key, and ſuppoſe the notes on $\frac{1}{5}$ and $\frac{1}{7}$ out of the queſtion, ſo that the notes you want will run on in a more eaſy and natural order, and the ſharp ſtring will alſo give the ſharp thirds and ſevenths all through. The like of other keys or pitches. In ſome pitches you may take only three ſtrings, and tune the other a fourth, fifth, eighth, a flat, or ſharp, or whatever you have occaſion for. Thus, if you pitch in 6, you may tune the fourth ſtring a fourth under, by which means you not only have that fourth more convenient than by going down to $\frac{1}{2}$, but have alſo the octave below the key, with other notes above, which ſome performances might require: or you may add a fifth, or ſixth ſtring, and reſerve them as by-ſtrings, for theſe and the like purpoſes.

ONE finger can very eaſily manage the notes on each fret or croſs line, as the ſtrings are not to be preſſed down, but the finger ſlipt as lightly over them as poſſible. The ſtrings ſhould be all of a ſize, or nearly ſo; not ſmall; and as even and clear toned as poſſible. If the inſtrument was longer than a violin (I mean on account of the ſtrings), and if it was made ſomewhat like a mandoline or guittar, perhaps the ſounds would be more melodious; ſuch an inſtrument would do very well to play harmonics all through with; and a maſter would not be at a loſs to play with it by turns (by means of ſhifting) in the common way alſo.

P. S. SINCE the note in page 69 was ſent to the Printer, I have ſatisfied myſelf that the harmonic ſounds which ariſe by bowing, depend entirely on the bow, as therein obſerved; for.

I. No ſuch ſounds ever ariſe by making the ſtring ſound by any other method that I can diſcover.

II. THE ſounds which ariſe depend entirely on the part of the ſtring bowed upon; and the part of the ſtring to be bowed on, in order to produce any given harmonic ſound, may even be *calculated*, by proceeding on the data delivered in the ſecond and third cautions in the theory above.

III. The bow therefore acts in a double capacity in theſe caſes, both ſtopping, and vibrating the ſtring, as before obſerved.

IV. The eye can very eaſily diſtinguiſh when a ſtring vibrates harmonically, and when only in the common way; in the latter caſe, the whole ſtring freely and viſibly forms the harmonic curve; in the former only its aliquot parts. Both theſe caſes may indeed happen together, as hath already been noticed, but then the latitude of the vibration of the whole ſtring is proportionally and even viſibly affected.

V. If Tartini's theory were juſt, the ſtronger the ſtring was made to vibrate, the louder would the harmonic ſounds be excited: but the contrary of this obtains; for in order to raiſe theſe ſounds we muſt bow very lightly, for if the bowing be ſtrong no ſuch ſounds are heard.

I COULD enter into a more ample refutation of this theory; but theſe few hints will be ſufficient to the philoſopher and mathematician. If any one chooſes to examine the matter by experiment, he will do well to obſerve that bowing with a common bow, and with a ſingle hair makes a very material difference; the former occupies a greater ſpace on the ſtring, and therefore raiſes more notes, and in a more irregular manner: but this is avoided by uſing only a ſingle hair.

I WOULD wiſh, however, not to be miſunderſtood. That a ſtring of an harpſichord, &c. when ſounded affects all thoſe ſtrings that are concords, I by no means deny; they do it on the ſame principle that one ſtring excites another which is in uniſon with it, and which is too well known to philoſophers to need explanation. But that a ſtring when ſounded raiſes alſo the various harmonic ſounds which that ſtring yields by vibrations in its aliquot parts, is, I think, ſufficiently refuted by what has been ſaid, or at leaſt could be refuted by purſuing theſe hints.

AN
INQUIRY
CONCERNING
COMBUSTION.

SECTION I.

The principal Phenomena of incombuſtible Bodies.

I. IF an incombuſtible body be expoſed to the focus of the ſun's light collected by a burning glaſs, to a culinary fire, to friction, or the like, it will become hot; and its heat will be greater, according to the power of the agent. The heat will continue as long as the cauſe continues to act.

II. BUT if that cauſe be removed, the body loſes its heat by degrees, till it becomes of an equal temperature with the ſubſtances around.

III. Bodies are expanded by heat, and contracted by cold; and different bodies in a greater or leſs degree, according to their denſity, the coheſion of their particles, and other circumſtances.

IV. If a ſolid incombuſtible body be heated, and another be applied to it cold, the former will communicate heat to the latter, and if the heat of the former be ſufficiently kept up, it will in the end cauſe the latter to be hot to any poſſible degree.

N. B. By *cold*, I mean a degree of heat leſs than that of the common temperature; and by *heat*, the contrary. But it is more philoſophical to uſe only the term *heat*, and to conſider bodies as more or leſs hot according as they raiſe or ſink the fluid in the thermometer.

V. When an incombuſtible body is heated to a proper degree, it emits light, ſo as to cauſe the body to appear luminous to the eye, the light increaſes with the heat; but if it be ſuffered to cool, the light decreaſes again with the heat; and when it arrives at about the ſame degree as when it began to ſhine, the light ceaſes to be viſible: if another body be applied to this when ſufficiently luminous, it will alſo acquire from it a luminous heat.

VI. BODIES heated till they become luminous, are ſaid to be *ignited.*

VII. SOLID bodies are rendered fluid by heat; and fluid bodies with ſufficient degrees of heat are turned into vapour. But different degrees of heat are requiſite to produce theſe effects on different bodies.

SECTION II.

The Phenomena of combuſtible Bodies.

HAVING premiſed as much as was judged neceſſary concerning the heat and light of uninflammable bodies, we may proceed to the ſubject of inflammable ones.

I. If a ſufficient heat be applied to a perfectly inflammable body *expoſed to the air*, it will kindle into a flame: but this flame does not require the aſſiſtance of the cauſe by which it was kindled in order to the continuance of its heat and light, as is the caſe with inflammable bodies. It has the property of maintaining or ſupporting itſelf till the whole of the body or ſubſtance is conſumed.

II. A body, or vapour, when heated as above is ſaid to be *red hot;* but the adjective ſhould be varied: and we may with equal propriety ſay that the flame of ſulphur is *blue hot*; the flame of copper *green hot*, and ſo of other colours. Uninflammable bodies, in the firſt degree of luminous heat, emit the red-making rays moſt copiouſly, and thence are ſaid to be red hot. If they are heated more violently, they emit all the rays in more equal proportion, and thence are ſaid to be white hot. Thus alſo the flame of ſulphur emits the blue-making rays moſt copiouſly; the flame of copper the green, and ſo of others; *ſhining hot*, therefore, would be a more proper general expreſſion.

III. In order that combuſtible bodies may burn, or flame, they muſt not only be expoſed to the air, but raiſed into *vapour*: and even the vapour thus raiſed muſt be put into a proper ſtate, otherwiſe no flame will be produced. Thus, ſpirit of wine may be evaporated entirely in open air, and yet no combuſtion happen.

IV. The chief circumſtance requiſite to the inflammation of a combuſtible vapour in open air, is *a due degree of heat*; if that be applied to the vapour when properly compreſſed by the atmoſphere, it inflames, in whatever manner the heat be communicated. The touch of an inflammable body already burning, or of an uninflammable

inflammable body ignited, is not neceſſary for that purpoſe.

V. Different combuſtible bodies require different degrees of heat to make them burn; for as they only burn by a flame, they muſt firſt be raiſed into vapour. But different inflammable ſubſtances require different degrees of heat to raiſe them into vapour according to their volatility: and even afterwards, this vapour is more or leſs difficult to be turned into flame, according as it is in its nature more or leſs combuſtible.

VI. In order that the combuſtion may be continued after once begun, without the aſſiſtance of extraneous heat, the body muſt be poſſeſſed of a ſufficient quantity of the inflammable principle, or *phlogiſton;* and then, if the other ingredients of that body be in due proportion, and ſufficiently volatile, the combuſtion will continue as long as any of the ſubſtance remains; as happens with alcohol. If the phlogiſton, though in ſufficient quantity, be combined with matter of a fixed nature, the aſſiſtance of extraneous heat is neceſſary to the combuſtion, as without it the particles of the body with which the phlogiſton is combined, cannot be duly expoſed to the action of the air: this happens with ſome metals. Vegetables, and moſt other combuſtible bodies partake of both theſe caſes. And even after the latter operation is carried as far as poſſible, a ſubſtance will remain which is a truly incombuſtible body.——The combuſtion in the former caſe is called *inflammation;* in the latter, *calcination.*

VII. A SHINING heat in the body of the matter to be burnt has, properly ſpeaking, no connection with its combuſtion. Thus iron is ignited before its combuſtion begins; ſulphur, on the contrary, burns before it has acquired that degree of heat. Burning phoſphorus cannot ſet fire to zinc; but zinc can inflame phoſphorus long before it has acquired even a luminous heat. Different ſubſtances require different degrees of heat to begin their combuſtion, as mentioned before: and if the due degree of that heat be applied, provided the vapour be ſufficiently inflammable, duly condenſed, and expoſed to the action of the air, the inflammation takes place, though the body by which it is communicated be neither in actual combuſtion, nor ignited.

VIII. It has been ſufficiently demonſtrated by philoſophers that combuſtible bodies contain a principle which *they call phlogiſton;* and that this conſtitutes the eſſential difference between combuſtible and incombuſtible bodies ; I ſay which *they call* phlogiſton, for they ſuppoſe that this principle is reſolved into elementary fire by combuſtion, and hence they account for the *heat* and *light* attending this proceſs : Dr. Black terms it, for this reaſon, *the principle of inflammability*, and others again, *the inflammable principle*. But it will appear, in the courſe of the following Eſſay, that the phlogiſton is a fixed principle, of a nature very different from what it has hitherto been imagined ; that it is not fire ; and that it is only *mediately* the cauſe of heat in combuſtion. For theſe, and other reaſons which will be ſeen in the ſequel, I would ſubmit to the learned whether any of the terms above mentioned ought to be continued ? and whether *electron*, or ſome other, ought not to be ſubſtituted in their ſtead ? I have, however, uſed the old word, till I have their approbation for adopting a new one.

SECTION III.

Of the Principle on which Combuſtion depends.

EXPERIMENT I.

IF alcohol be evaporated with an heat not ſufficient to inflame it, and the vapour be condenſed, it will be found the ſame ſubſtance as before.

Exp. II. If the vapour of alcohol be inflamed, and what flies off condenſed, it will not be found to be alcohol, nor even an inflammable ſubſtance; for nothing but water can be diſcovered in it.

Corollary I. By inflammation, therefore, the vapour of alcohol is decompoſed: and this holds good with all inflammable vapours.

Exp. III. If the wick of a candle be ſet fire to in open air, the flame will continue until the candle is burnt.

EXP. IV. BUT if it burns only in a certain quantity of air, the combuſtion will continue only during a time; which will be greater according to the quantity of air. If this air be exchanged for freſh, and the candle again lighted, it will burn only about the ſame time as before. By changing the air a ſufficient number of times, the whole candle may be burnt out as completely as if it had not been confined in a cloſe veſſel: but no art can continue the combuſtion without ſuch renewal of the air.

COROL. II. THE ſecond experiment ſhewed that inflammable vapours are decompoſed by combuſtion, and reduced to the ſtate of *uninflammable bodies*. They were, therefore, decompoſed by having their *phlogiſton* taken from them. In this experiment, we find that the air takes ſomething from the burning vapour; for after a vapour has burnt in a given quantity of air during a ſufficient time, the combuſtion cannot any longer be continued; though if freſh air be added, it may; the air therefore was ſaturated with ſomething which it had taken from the inflamed vapour; but what the vapour loſt was the principle which conſtituted it an inflammable ſubſtance. It was the *phlogiſton* therefore which the air took from the vapour, and with which, in the end, it was ſaturated. Now, as the flame continued only while the air was taking the phlogiſton from the vapour, and went out when the air was no longer able to do this, it ſeems "that the combuſtion depended entirely on ſuch action of the air on the phlogiſton."

Exp. V. If a bit of charcoal be incloſed in a large veſſel, and made ſufficiently hot, and then the whole be ſuffered to cool, the air in the veſſel will be found ſatured with phlogiſton. If freſh air be added to the coal (the firſt being withdrawn), the operation repeated, and ſo on ſucceſſively for a number of times, the phlogiſton of the coal will be very ſenſibly diminiſhed, as I have tried. And, therefore, if the operation had been repeated a ſufficient number of times, the whole of the phlogiſton might have been extracted as completely as if it had been burnt in the open air.

Corol. III. A coal is a combination of phlogiſton with earth; but by this experiment it appears "that the phlogiſton has a greater affinity with air than with the earth of the coal; and therefore when the proper circumſtances concur, it quits the latter to join with the former." The circumſtances which attend this are ſimilar to what happens in other chymical decompoſitions. If I put a quantity of fixed alcali united with ſome other ſubſtance, ſuppoſe ſulphur into a glaſs, and pour on it a little vinegar, the vinegar will extract the alcali from the ſulphur until it is perfectly ſaturated therewith; but even if heat be afterwards applied, it will not extract any more; neither will air, when ſaturated with phlogiſton, extract any more of that principle from the charcoal. If now the ſaturated acid be ſeparated, and freſh poured on, more alcali will be taken from the compound; and thus we may proceed till the whole is drawn out; the ſulphur will then remain behind, deprived of its alcali, in the ſame manner as the earth of the coal remained behind deprived of its phlogiſton. The ſtrength of the analogy will eaſily be perceived by the philoſophical reader.

THE air ſaturated as above is called *phlogiſticated* and *fixable air*.

Exp. VI. If in open air any heat be applied to *ſulphur* below a certain degree, it will not not burn. If ſulphur be incloſed in a veſſel with fixed air, and a greater degree than that with which it would be burnt in the open air be applied, it ſtill remains uninflamed, and the ſame ſulphur; but if the veſſel with the ſulphur in this ſtate be uncovered, it kindles into a flame immediately on the admiſſion of air, without the application of a body already burning, and is entirely decompoſed.

Corol. IV. The ſubſtance which has hitherto been conſidered as having one of the greateſt degrees of affinity with phlogiſton is the vitriolic acid; for moſt other ſubſtances which contain that principle, part with it to this acid, when the circumſtances requiſite to their union concur; the ſubſtance formed by their union is *ſulphur*, the ſubject of the above experiment. But it appears that phlogiſton has a greater affinity with *air*, than with *vitriolic acid:* for, when the proper circumſtances concur, it quits the latter to join with the former. The affinities of phlogiſton therefore, with reſpect to theſe ſubſtances, ſhould be placed thus:

Phlogiſton,

Air,
Vitriolic Acid,
&c.

Now that the phlogiſton is really united with the air, by means of a ſuperior affinity, as explained above, appears not only from what has been ſaid, but alſo from this conſideration, that the air thus combined is altered in its ſpecific gravity, is leſs elaſtic, and in other reſpects changed in its properties. The properties of the vitriolic acid are likewiſe altered on its combination with the ſame principle, with an alcali, or any other ſubſtance. The ſaturation of air with phlogiſton, is as analogous to the ſaturation of the vitriolic acid with the ſame principle as any two proceſſes of the kind can be, allowing for the very different natures of theſe ſubſtances; but the following analogy will ſet it in a ſtill clearer light. Common ſalt, and cubic nitre, may, in this view, be conſidered as ſimilar, except in the attraction which their alcaline baſes have with their reſpective acids. The vitriolic acid decompoſes the former with greater eaſe than the latter, becauſe its principles are united by a weaker attraction. In like manner, phoſphorus and ſulphur may be conſidered as differing from each other only in the affinity which the phlogiſton has for the reſpective acids. But the air decompoſes phoſphorus with greater eaſe than ſulphur: and for this no other reaſon appears but that the phlogiſton has a weaker affinity with the phoſphoric than with the vitriolic acid. *In the proceſs of combuſtion, therefore, we muſt reaſon in the ſame manner as on other chymical affinities and decompoſitions.*

SECTION IV.

Of the Phlogiſton.

THE doctrine of combuſtion which at preſent prevails is, that the phlogiſton is *combined elementary fire*: that in this proceſs it is ſet at liberty, and reſumes its elaſtic ſtate; and that the heat and light of flame, as alſo the property which it has of ſupporting or maintaining itſelf, proceeds from the avolation of this diſengaged principle.

THE phenomena of fixed air are made uſe of to illuſtrate this theory, and from hence, indeed it was, by analogy, derived. Fixable air may be combined with various ſubſtances, and form with them groſs bodies: thus it may be combined with the cauſtic volatile alcali; it may be transferred from thence to other ſubſtances with which it has a greater affinity, to the cauſtic fixed alcali for example; and from thence again to quicklime. But if a ſubſtance be applied to the compound with which the quicklime or alcali has a greater affinity than with the air, it lets go the air, and unites with that ſubſtance. The air thus diſengaged, and no other ſubſtance at hand with which it can unite, reſumes its elaſtic ſtate, and becomes permanent air; as it flies off it cauſes an efferveſcence in the liquid in which it was contained.

It is ſuppoſed that elementary fire may, in like manner, be combined with bodies, and that it may be transferred from theſe to others for which it has a ſtronger attraction: thus, it may be combined with the earth of charcoal. From thence it may be transferred to metallic * calxes; from theſe to the phoſphoric acid, and from theſe again to the vitriolic. In the proceſs of combuſtion, it is conſidered as "let go by the body with which it was combined; that it reſumes its elaſtic or expanſive ſtate, and, by its flight, produces the phenomena of heat, &c. after the ſame manner as air produces effervеſcence." This theory is ingenious, but I think not true, for the following reaſon:

It is known to chymiſts, that pure ſulphur is a combination of the vitriolic acid with phlogiſton,

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* Zinc, &c.

giſton, and that theſe are the only ingredients which enter into the compoſition of that ſubſtance. In combuſtion the ſulphur is decompoſed by means of a third body, or air; which having a greater attraction for one of the ingredients than that which is already combined with it, that attracted ingredient quits the other, and unites with the air. Now if the analogy held good, we muſt ſay "that the vitriolic acid had a greater affinity with the air than with the phlogiſton, and therefore let go the latter to join with the former; that the phlogiſton thus diſengaged, reſumed its elaſtic ſtate as elementary fire, and cauſed by its flight the phenomena of heat, &c. juſt as diſengaged air cauſeth efferveſcence." But the reverſe of this happens, for the acid is left behind; and of courſe "it was the other ingredient, or phlogiſton, which combined with the air."

STRANGE as it may ſeem, this laſt fact is acknowledged by thoſe very perſons who embrace the theory of combuſtion above explained; ſo that it is matter of ſurpriſe that they have not diſcerned the perfect analogy of this proceſs with other chymical ones of the like kind.

Perhaps the light which attends combuſtion has *dazzled their eyes*, and proved an *ignis fatuus* which has led them out of their way. Perhaps alſo, the property which inflammable bodies have of maintaining the combuſtion once begun in them, has proved their ſtumbling block. For we ſhall find that theſe phenomena admit of an eaſy ſolution from the doctrine above laid down.

THE *deſtruction* * of the phlogiſton in this proceſs has been a favourite doctrine ſince Stahl honoured it with his opinion. But when vinegar extracts the alcali from liver of ſulphur, as in an experiment before related, chymiſts do not ſay that the alcali is deſtroyed, as they ſay that the inflammable principle is deſtroyed by combuſtion; their ideas on that head are clear enough: they rightly conclude that the alcali had left the ſulphur to unite with the vinegar. They argue in the ſame manner on the decompoſition of charcoal by the vitriolic acid! why this reaſoning ſhould have been departed from in the inſtance before us, is not eaſy to imagine.

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* Vide Macquer's Chymical Dictionary.

It would be eaſy to bring other objections to the prevailing theory of combuſtion; but as I imagine that firſt ſtated to be an *argumentum crucis* (if the expreſſion may be allowed), it will be needleſs to trouble the reader with more.

Part of this theory however is true, as will hereafter appear. The *light* of flame proceeds from the diſengaged phlogiſton, though the *heat* does not. Alſo the *heat* really proceeds from diſengaged fire, as chymiſts at preſent imagine; they are only miſtaken as to the origin of that fire.

SECTION V.

Of the Heat and Light attending Combuſtion.

IT is well known to chymiſts that when certain bodies unite, their combination is followed by a greater heat than what thoſe bodies poſſeſſed before. Thus heat is generated when an acid is ſaturating an alcali. The like happens when water is mixed with ſpirit of wine; and a ſtill greater heat follows on mixing water with the mineral acids. In ſome caſes therefore the heat may perhaps be ſo great that the new compound ſhall be luminous. When water is mixed with quicklime, the heat is ſometimes ſo great as to kindle combuſtible bodies with which it happens to be in contact. In the pyrophyrus of Homberg, the union of water with the vitriolic acid is attended by ſo great an heat, that the inflammable ſubſtances in the compound are ſet on fire by it; and the nitrous acid and oils actually inflame.

The cauſe of this heat I defer the conſideration of to a future ſection: the fact alone is ſufficient for our preſent purpoſe. The heat generated by the combination of phlogiſton and air may, for the preſent, be reckoned analogous to theſe; and, in ordinary combuſtion, is ſo great as to be luminous, as may be gathered from the ſecond Corollary in the third ſection hereof.

It may farther be obſerved that combuſtible bodies are heated to a degree before they begin to flame: and it may be preſumed that the ſame quantity of heat is generated by the combination whether the particles before their union were hotter or colder. When, therefore, the ingredients are previouſly heated, or their temperature is higher; the heat after their combination will be greater than if they had united in a colder ſtate, becauſe the heat generated by their union, is added to that which they had before acquired. If this be joined with the conſiderations in the preceding paragraphs, it will ceaſe to be a wonder that this proceſs is attended with a *ſhining heat*.

Now, in caſes where the heat is not intenſe, as in the combuſtion of ſulphur, I apprehend that the combined phlogiſton and air only are luminous: but if it be ſufficient, a ſhining heat will be generated in the extraneous particles of the vapour. I defer a particular conſideration of the light of flame to a future ſection, and ſhall here only obſerve that a vapour whoſe particles are rendered luminous, muſt appear to us under the form of *flame*.

SECTION VI.

Of the Continuance of Combuſtion.

IN the foregoing pages we have endeavoured to explain the principle on which combuſtion depends, and the phenomena of heat and light which attend the combuſtion of inflammable bodies. The property which theſe bodies have of maintaining the combuſtion after it is begun, ſhall be the ſubject of this ſection. I ſhall treat this ſubject in as conciſe a manner as I did the others; and an attention to the following circumſtances will ſufficiently explain my ideas on that head.

LET a vapour be raiſed from a perfectly inflammable ſubſtance in open air, let that vapour be properly compreſſed by the atmoſphere, and a ſufficient heat applied, the particles of air will attract the phlogiſton from the particles with which it was before united, and a ſhining heat will follow. The vapour therefore will appear under the form of *flame*. The particles thus ignited will be enabled to communicate heat to thoſe on the ſurface ſufficient to raiſe them into a vapour proper to be acted on by the air. This vapour being in like manner decompoſed, theſe freſh ignited particles of phlogiſton and air will communicate heat to thoſe next on the ſurface, which therefore will likewiſe be elevated and decompoſed; and ſo on in a continual ſucceſſion, as long as any of the ſubſtance remains: for, as by the combination of each particle of air with phlogiſton, heat is generated, and that in the great degree mentioned above, ſo many particles as thus combine, ſo many new ſources or ſprings of heat will there be; which, with what follows, will be amply ſufficient to account for the phenomenon in queſtion.

For this proceſs is aſſiſted or facilitated by the action of the atmoſphere, by which the vapour is compreſſed, and the particles of air and phlogiſton forced into contact: hence the combuſtion goes on in an heavy atmoſphere better than in a light one: Hence alſo, when bodies burn in cloſe veſſels, the flame ceaſes before the air is all ſaturated with phlogiſton, becauſe its elaſticity being weakened, the vapour is not ſufficiently compreſſed. Bellows, and currents of air, beſides that they drive away the ſaturated air, and apply freſh particles to the vapour, aſſiſt combuſtion on this principle.

It appears, from what has been already ſaid, that different combuſtible bodies require different degrees of heat to make them flame. The degree of heat therefore which is neceſſary to begin the combuſtion will, for the ſame reaſon, be required for its continuance; now, whatever this requiſite degree of heat be, yet if the body be perfectly and uniformly inflammable, or burns wholly away when once kindled, it will be found that *more phlogiſton and air are combined in a given time;* and therefore a greater quantity of heat continually generated, ſufficient to equal that firſt degree of heat, and of courſe to maintain the combuſtion. We have, therefore, from the above principles, a very eaſy ſolution of the phenomena of combuſtion, and the theory will perhaps be the more readily embraced, as its principles are within the bounds of common obſervation.

THE reader will eaſily apply the doctrine delivered to the particular phenomena of combuſtion; with an inſtance of which I ſhall conclude this ſection.

EXPERIMENT. If, inſtead of air, *nitre* be mixed with a combuſtible body, and put into a cloſe veſſel in vacuo, or otherwiſe, and then made ſufficiently hot, the combuſtion of that body will be as complete, as if it had been burnt by means of air.

THE nitrous acid, therefore, contains a quantity of air of the ſame nature with that of the atmoſphere, but in a combined ſtate: as ſoon as the proper circumſtances concur, the phlogiſton in the inflammable body, and the air in the nitrous acid, by a mutual attraction are withdrawn from the ſubſtances with which they were before combined, and unite with a ſhining heat, in the ſame manner as atmoſpherical air and phlogiſton in the inſtances deſcribed. The air thus at liberty, reſumes its elaſtic ſtate, and becomes the ſame fixed air as that generated by common combuſtion. The phenomena of gun-powder and other nitrous combuſtions may be underſtood by means of this theory.

CHYMISTS have obſerved, that if phlogiſton be combined with the phoſphoric or vitriolic acids, *ſulphurs* are formed: as nitrous acid has a greater affinity with phlogiſton than either of theſe, they conclude that a ſulphur is likewiſe formed by their combination. "But (ſay they) the nitrous acid and phlogiſton unite with ſuch violence, that the ſulphur is deſtroyed the very inſtant that it is formed *." Is not the fixable air produced in this proceſs the nitrous ſulphur? and would it not be conſiſtent with chymical analogy, and therefore more proper, to call fixable air in general Aërial Sulphur? but this is ſpoken with ſubmiſſion to better judges.

IT has generally been ſuppoſed that the great attraction which the nitrous acid has for phlogiſton, is owing to its containing that principle as *a conſtituent part* †. But the reaſon now appears to be that it contains common *atmoſpherical*

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* Macquer's Chymical Dictionary. † Ib.

rival air; I take it that it contains phlogiſton in no other manner than as the volatile vitriolic acid does; and that by expoſing to the air the phlogiſton is diſſipated; for it ceaſes to fume, and becomes fixed like the oil of vitriol. Perhaps air conſtitutes the *eſſential part* of the nitrous acid, on which its taſte, corroſiveneſs, and other general properties depend; and it ſeems to me that it is combined with nothing but water by means of an earth: for water only is to be found in the nitrous clyſſus, and and the earth may be left behind with the alcali. I uſed to think that it was combined with water alone: but if the reaſoning in a following ſection on air be admitted, that cannot be the caſe; for water parts with air with a leſs degree of heat than nitre does, and therefore there muſt be ſome other ſubſtance of a more fixed nature with which it is combined, and only mediately with the water by means of that ſubſtance. Is the eſſential part of the *vitriolic acid* alſo atmoſpherical air in a ſtate of combination *, but combined with ſuch ſubſtances, or in ſuch a manner, that it is not ſeparable by the phlogiſton, as in the nitrous? and is this the reaſon of its great affinity with phlogiſton? and may the like query be put concerning the other acids? but this by way of digreſſion.

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* Do not the exploſions which have been obſerved of balſam of ſulphur favour this opinion? did the air and phlogiſton mutually diſengage each other, and form fixed air?

FROM what has been ſaid, it appears that the phenomena of combuſtion depends on this principle; that air has a greater affinity with phlogiſton than the ſubſtances have with which it is combined in inflammable bodies, and therefore when all circumſtances properly concur, it attracts that principle from thoſe bodies; that a ſhining heat is generated by their combination; and that this decompoſition when once ſufficiently begun in a perfectly inflammable body, together with the ſhining heat which is a conſequence thereof, will be continued on the principles above laid down, without any farther aſſiſtance from extraneous heat, as long as any of the ſubſtance remains.

WHEN phlogiſton is combined with the earth of charcoal, with the vitriolic acid, or certain other ſubſtances, a combuſtion may happen by reaſon that the air can attract it from theſe ſubſtances; but when it is combined with air no combuſtion can happen, becauſe air cannot attract phlogiſton from other air, any more than the vitriolic acid can attract it from ſulphur; for the affinities are equal, and one ſubſtance cannot attract another from a third, with which it is combined, but by means of a ſuperior affinity. This may alſo be applied to the nitrous ſulphur above ſpoken of.

There are however, certainly, ſubſtances capable of attracting the phlogiſton from air, otherwiſe the whole atmoſphere would in time be converted into fixable air. The ingenious Dr. Prieſtley, to whoſe labours the learned world is ſo greatly indebted, has already diſcovered two of theſe means: he ſhews that fixed air may be rendered pure by vegetables growing in it, and by water. It may perhaps be added, that as fixable air has a greater ſpecific gravity than common air, and therefore naturally tends downwards, it enters into the earth, and combines with ſuch bodies as may be diſpoſed to receive it. That ſome ſubſtances may have the property of depriving it of its phlogiſton, which then enters into their compoſition; and perhaps, in ſome caſes, there may be no other method of combining that principle with bodies, at leaſt in certain manners. The phlogiſton, therefore, when combined with air, ſeems to be in the moſt proper ſtate for certain intimate combinations of it with vegetable and other ſubſtances; as it is probably reduced to its integral parts. Hence we have ſome idea not only of the manner in which fixable air is deprived of its phlogiſton, but alſo of the circulation of the laſt mentioned principle, from bodies to air, and from air again into bodies. Perhaps alſo the fixed air, when deprived of its phlogiſton, may, in ſome caſes, be converted into acids, if the above queries concerning acids be true; but theſe things remain to be inquired into.

SECTION VII.

A Speculation[*].

AIR has uſually been reckoned a fluid *ſui generis*, and called, in contradiſtinction to others which are coherent, an *elaſtic fluid*.

I HAVE long been of opinion that the elaſticity of air depends on heat; for if the heat be increaſed, the elaſticity is increaſed; if it be

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[*] In the Eſſay on the ſenſes I allotted a ſection for ſuch hints and conjectures as had occurred to me on thoſe ſubjects, in order to their being further inquired into by others: I ſhall devote this ſection to a like purpoſe. As I do not pretend to offer theſe conjectures as demonſtrated truths, any errors will be pardoned by the candid, as the detection of them by experiments may lead to real diſcoveries; I have, for my amuſement, carried the ideas contained in this ſection, as well as others which are not mentioned, to greater lengths. If this ſhort extract be approved, I may in a future edition render it more copious. Thoſe, however, who do not reliſh ſpeculative reaſoning may paſs over this ſection.

diminiſhed,

diminiſhed, the elaſticity is alſo diminiſhed in a certain regular proportion: it ſhould ſeem, therefore, that if air was entirely deprived of heat, its particles would become coherent.

When water is raiſed into vapour it is alſo elaſtic, and its elaſticity is greater as more heat is afterwards applied. The vapour of water, therefore, is of a ſimilar nature to air: the only difference, in this reſpect, between them is, that water requires a vaſt deal more heat to render it elaſtic. The like may be obſerved of other bodies *.

If we imagine two particles in contact, and that heat be applied, the heat will force them to quit each other. The particles, while within the ſpheres of their cohering forces, will reſiſt the action of heat more, as their cohering forces are ſtronger. But when the heat becomes ſo great as to force them beyond thoſe ſpheres, they will be elaſtic, like air; their elaſticity will be greater as the heat is increaſed, and that in a certain regular proportion; as mentioned above.

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* It ſeems to me that this propoſition may be made as general as Mr. Braun's concerning the fluidity of bodies.

Case I. Heat, therefore, is the cauſe of the repulſive affection among particles of air; and if this cauſe be removed, the particles have no ſuch tendency.

Case II. Fire ſurrounds a particle of air in the manner of an atmoſphere; it is denſer near the particle, and rarer at a diſtance from it; and hence the repulſive power of particles of air. Fire therefore is attracted by the particles of air.

Case III. When air is hotter it is more elaſtic; that is, its particles are ſurrounded with greater and denſer atmoſpheres of fire, and therefore their repulſive powers are ſtronger.

Case IV. As our atmoſphere, by its gravity, is denſer, or more compreſſed near the earth, than at a diſtance from it, and that in a direct proportion; ſo by the gravity of fire towards a particle of air, the fire is in like manner compreſſed. The denſity diminiſhes according to the diſtance from the particle; and hence the repulſive force of particles of air is directly as the diſtance of theſe particles from each other.

Case V. When the quantity of fire compoſing the atmoſphere of a particle is greater, the compreſſion or denſity of the fire near the particle will be greater, juſt as happens with our air: and therefore the repulſive force of the particle will be increaſed with the heat, and will alſo reach to a greater diſtance *.

Case VI. If two ſimilar particles of air, but with unequal atmoſpheres, be brought ſufficiently near to each other, the particle which has the greateſt atmoſphere will part with fire to that which has leaſt, till their quantities are equal. The like may be obſerved of other homogeneous particles.

The reaſon of this is obvious; the attraction for fire, or the gravitation of fire towards them, being equal in both or all the particles.

Case VII. If by any means the gravity of fire towards a particle, or, if you will, the attraction

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* It is obvious from hence, that the repulſion at the ſame diſtance from the particle does not increaſe in a direct proportion with the heat, but in a ratio which will eaſily occur to the mathematician.

traction of a particle for fire be weakened, the atmoſphere which that particle retains at the ſame common temperature will be leſs in proportion thereto, and it will alſo be leſs denſe; ſo that its repulſive power will likewiſe be diminiſhed.

THE idea on which this caſe is founded was ſuggeſted to me by the following conſideration.

IT has long been known that lead by calcination acquires conſiderable weight; M. Margraaf has diſcovered that the acid left behind after the combuſtion of phoſphorus is almoſt half as heavy again as the phoſphorus employed: and yet in both theſe caſes, many particles muſt have eſcaped beſides the phlogiſton. I am told that Dr. Black has made experiments on metals with acids which agree ſo well with theſe that he is convinced of the truth of the inference which has been drawn from them, viz. "That the gravity of bodies is diminiſhed by their combination with phlogiſton."

THE cauſe of gravity, as conjectured by Sir Iſaac Newton, and now generally ſuppoſed by philoſophers, is a very ſubtile elaſtic medium, which is rarer near a particle of matter, and denſer at a diſtance from it. That therefore two ſuch particles will be mutually impelled by the denſer, towards the rarer parts of this medium, and in courſe towards each other.

As the æther is *rarer near*, and *denſer at a diſtance from*, *a particle*, it ſhews that there is a *mutual repulſion* between the particles of bodies and this fluid.

That, therefore, according as this *mutual repulſion* is greater, the rarity of the medium near the particles, and the force of the particles' gravity, will be greater; and as the *mutual repulſion* is leſs, the æther near the particles will be leſs rare, and their gravity diminiſhed.

It therefore appeared to me, that as phlogiſton when combined with the particles of bodies diminiſhes their gravity, it does it *by weakening the repulſion between theſe particles and æther.*

Till now, I had imagined, with Dr. Black*, and others, that fire, æther, and phlogiſton were one and the ſame fluid: but on applying this reaſoning to my notion concerning the repulſive force of the particles of air, I found that it entirely claſhed with it; for phlogiſton weakens the elaſticity of air. And now, for the firſt time, it occurred to me that the diſpoſition of particles of bodies towards æther and fire are quite oppoſite. For whereas fire gravitates towards, or is attracted by thoſe particles, æther on the contrary is repelled; and this alſo led me to conſider that æther cauſes the gravity or attraction of particles towards each other; fire, on the contrary, their repulſion. It followed, therefore, that if phlogiſton diminiſhed the repulſion between the particles of bodies and æther, *and thereby leſſened the mutual gravity of theſe particles;* it muſt on the contrary *diminiſh the attraction between theſe particles and fire*, *and of courſe weaken their mutual repulſion.*

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* To ſome notes which a friend had taken while attending the lectures of that great phyſician and philoſopher (I wiſh they

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they had been more perfect), and to extracts from Dr. Prieſtley's diſcoveries concerning air, I owe my having been enabled to work this latter part of my Eſſay into its preſent form.

THIS ſuggeſted to me another idea. If particles, when combined with phlogiſton, have their attraction for fire diminiſhed, it ſhould follow, that the ſame quantity of fire added to a phlogiſticated, and an unphlogiſticated particle, would heat the former moſt; becauſe it would be leſs forcibly retained by that particle than by the other, and therefore a greater quantity would be communicated to a third and colder particle applied. To ſatisfy myſelf with regard to the juſtneſs of this idea, I made the following experiments.

EXPERIMENT I. In an iron pot filled with ſand, heated over the fire, I placed, at a ſmall diſtance from each other, two gallipots, one of which contained water, the other lamp-oil, ſo that they were in equal degrees of heat; after they had remained ſome time, I found that the oil had acquired a greater heat than the water.

EXP. II. I made the ſame experiment with minium and lead, and found that the metal acquired a greater heat than the calx.

Exp. III. I tried the ſame with ſeveral other ſubſtances, whoſe ſpecific gravities would admit of the experiment, with the like reſult.

I endeavoured to make the experiment with common and fixable air; but the fluids being ſo rare, and not properly confined, and the heat of the containing veſſels ſo much interfering, I could do nothing to my ſatisfaction; and therefore could only infer from analogy; for the only thermometers that I uſed were conic ſcurvy-graſs phials, and Daffy's elixir bottles, with ſpirit of wine in them, the ſurfaces of which I marked on the outſide with ink. The three firſt experiments however, and the conſideration that fixed air is leſs elaſtic than common air, ſeemed ſufficiently to eſtabliſh the truth of the propoſition which they were intended to determine.

So then inſtead of æther, fire, and phlogiſton being the ſame, as is at preſent ſuppoſed, they appear to be three diſtinct and very different fluids; and their relations or affections towards the particles of bodies and each other, ſeem to be as follows.

I. SINCE when the particles of bodies contain leaſt phlogiſton, fire gravitates towards them moſt, there is a mutual attraction between the particles of bodies and fire.

II. AND ſince when the particles of bodies are moſt free from phlogiſton, æther avoids them moſt, there is a mutual repulſion between thoſe particles and æther.

III. PHLOGISTON and æther therefore mutually attract each other; and on the contrary.

IV. THERE is a mutual repulſion between phlogiſton and fire.

PHLOGISTON therefore is to æther, what the particles of bodies are to fire: for as fire proceeds from denſe to rare in its progreſs from the latter, ſo æther proceeds from denſe to rare in its progreſs from the particles of the former. It ſeems then that there are two different, and as it were oppoſite kinds of ſubſtance of which bodies are compoſed, and two elaſtic fluids anſwerable to them. And if this be true, we muſt reckon four general principles, viz. *æther*, *fire*, *phlogiſton*, and *the particles of bodies;* but as this laſt name is too general, ſuppoſe we call what is meant by them *earth*. Perhaps, however, æther and phlogiſton may be compoſed of the ſame matter, and ſo may fire, and earth: perhaps all four may be only different modifications of the ſame ſubſtance; for we muſt go beyond fire and æther before we arrive at the ultimate principles of nature. There may be ſtill ſubtiler principles on which the elaſticities, and other properties of theſe depend.

THESE four ſubſtances may perhaps be conſidered as the *four elements* with more propriety than thoſe of Ariſtotle which ſo long prevailed, and perhaps the phenomena of nature may be better underſtood by means of them than they are at preſent.

THE proportion of earth in nature ſeems to be much greater than that of phlogiſton, and the proportion of æther much greater than that of fire: the gravity of bodies towards the earth, I think, proves this.

ALSO phlogiſton appears to be much more ſubtile than earth, and æther than fire; for the elaſtic fluid formed by particles of earth with atmoſpheres of fire, is much groſſer than that formed by particles of phlogiſton with atmoſpheres of æther. Thus, light is more ſubtile than air, and electricity than fixable air; the analogy between theſe may hereafter be ſhewn.

PARTICLES of phlogiſton freeſt from earth attract and retain the greateſt atmoſpheres of æther; and particles of earth freeſt from phlogiſton attract and retain the greateſt atmoſpheres of fire. Hence particles of earth, when moſt free from phlogiſton, are moſt elaſtic or repulſive; and ſo are particles of phlogiſton when freeſt from earth.

THOUGH earth attracts fire, which is repelled by phlogiſton, yet there is a ſtronger attraction between phlogiſton and earth, than between any other two of the principles.

ONE particle of earth cannot cohere with another, unleſs one or both be previouſly combined with a ſufficient quantity of phlogiſton. The phlogiſton both attracts the particles of earth, and diſperſes their atmoſpheres of fire, which kept them aſunder *. Earth, for a like reaſon, is the principle of coheſion between the particles of phlogiſton.

SUPPOSE a particle of earth, and another of phlogiſton, with each its proper atmoſphere of fire and æther, if they could be forced into combination, they would quit their attractions for æther and fire, and exert their forces on each other; or they would loſe ſo much of their attractions for thoſe mediums (and therefore of the atmoſpheres) as they exerted on each.

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[*] If phlogiſton be ſuppoſed to compoſe the cohering forces of the particles of earth, the ætherial atmoſpheres of the particles of this phlogiſton, though greatly decreaſed by being combined with earth, will yet extend to a little diſtance beyond the cohering atmoſphere ſufficiently ſtrong for producing a ſenſible effect, and will furniſh us perhaps with the cauſe of the repulſive force obſerved by Sir Iſaac Newton, viz. that two object glaſſes will lie on one another without touching; that two poliſhed marbles are with difficulty made to cohere; that beyond the cohering forces of bodies there is a repulſion; that the rays of light are inflected, reflected, and refracted by bodies, and the like. The leſs forcibly the phlogiſton is combined, the greater muſt theſe effects be.

But pure earth and phlogiſton cannot directly combine, by reaſon that their atmoſpheres hinder their union. Thus light cannot be combined directly with air; but if the light be preſented to a particle of earth already combined with a proper quantity of phlogiſton, whereby its atmoſphere of fire may be ſufficiently diminiſhed, the light can enter into combination with it, and then air, by reaſon of a ſuperior attraction, can take it from that particle. The like may be obſerved of phlogiſton previouſly combined with earth: The ſolution of ſome curious phenomena ſeem to depend on this principle, as may hereafter be ſhewn, if this ſpecimen be approved.

It may be proper to obſerve that there is not a perfect analogy between earth and æther, and phlogiſton and fire, as may at firſt view be imagined. The quantity of fire in the univerſe ſeems to be very ſmall, and to be only confined to the planets and other heavenly bodies; round the earthy particles of which it forms atmoſpheres, as hath been deſcribed, and perhaps of no very great extent even in particles of air. But æther is in quantity vaſtly ſuperior; and as its great ſuppoſer ſays, "is expanded throughout all the heavens;" particles of earth therefore will have, beſides their limited, decreaſing, or repulſive atmoſpheres of fire, increaſing or attractive atmoſpheres of æther, extending perhaps to the utmoſt bounds of the univerſe. But particles of phlogiſton will have decreaſing or repulſive atmoſpheres of æther, reaching to the ſame diſtance as theſe laſt, but no increaſing or attractive atmoſpheres of fire, or at leaſt only momentary ones; becauſe, on account of the ſmall quantity of this medium, and its not filling the univerſe, it will all be attracted by, and gathered about the particles of earth. To illuſtrate this it may be noted, that when to a particle of earth another of phlogiſton is added, part of its repulſive atmoſphere of fire is diſlodged. Now, if the analogy held good, the diſlodged fire ought to go into the increaſing or attractive atmoſphere of fire of the particle of phlogiſton added; but, on the contrary, it goes into the repulſive atmoſpheres of fire of the particles of earth around, as is proved by the thermometer and the ſenſe *. On the contrary, when to a particle of phlogiſton another of

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* Vide caſe XI.

earth is added, the æther which is expelled from the repulſive atmoſphere of the former, goes into the attracting atmoſphere of the latter. For whenoil of vitriol is mixed with water, fire is diſlodged from the particles of earth; and by the ſame reaſon æther muſt be diſlodged from the particles of phlogiſton. Now heat is cauſed by the diſlodged fire, becauſe it goes into the repelling atmoſpheres of fire of the ſurrounding particles of earth, as obſerved above *. But if the ſame rule held good with the diſlodged æther, the mixture would weigh heavier than the ingredients did before, becauſe the particles of phlogiſton having leſs repulſive atmoſpheres of æther, would be leſs repelled by the globe of the earth, and therefore they would have leſs levitation, or centrifugal force. But the æther diſlodged from the repulſive atmoſpheres of the particles of phlogiſton, goes into the attracting atmoſpheres of the particles of earth with which the phlogiſton is combined, and which therefore by that combination had its repulſion for æther diminiſhed; and therefore what the particles of phlogiſton loſt in centrifugal, thoſe of earth loſt in centripetal force, ſo that the weight continued the ſame *. When theſe ideas firſt occurred to me, I made experiments with oil of vitriol and water, and with ſpirit of nitre and ice, to ſee whether they altered in weight after mixture. By the inaccuracy of my weights I had like to have fallen into an error, for the vitriolic mixture ſeemed heavier, and the nitrous lighter than their ingredients; but by repeating the experiment I diſcovered the cauſe to be in the weights. The abſolute gravities of the compounds were the ſame as thoſe of their ingredients; and conſequently the decreaſe or increaſe of attraction of æther by the particles of phlogiſton, was balanced by an

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* Perhaps Phlogiſton does not repel fire atmoſpherically, but by particle and particle, their ſubtilty being nearly alike. Or do they really *repel*, or only *expel* one another, as the fixed alcali expels the volatile from acids?

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* Imagine a particle of phlogiſton where gravity is -1, and another of earth whoſe gravity is $+2$. If they are combined, their gravity will be equal to the ſum of their gravities before combination, or $+1$: and this will be the caſe whether their combination be more or leſs intimate; and whether free particles with their full atmoſpheres be ſuppoſed, or particles already combined; for on their ſeparation from their previous combinations, they will inſtantly acquire their proper ætherial atmoſpheres, as is obvious from what has been ſaid.

equal increaſe or decreaſe of the repulſion of that medium by the particles of earth.

The principles which have been propoſed are, probably, of general extent. All bodies may be compounds of phlogiſton and earth, with regulating atmoſpheres of æther and fire; and all the differences in theſe bodies may ariſe from the different proportions and manners of their combination. A new field of ſpeculation ſeems therefore to be opened to philoſophers by this theory.

Case VIII. But if the mutual attraction be increaſed, the atmoſphere of the particle, and alſo its repulſive power will be augmented.

Case IX. Suppoſing two particles of air A and B, and that the gravity of fire towards B is decreaſed, ſo as to be but half of that towards A; the atmoſphere of A will contain twice the quantity of fire of the atmoſphere of B. If theſe two particles be brought near to each other, the atmoſpheres will not become equal, as in caſe VI. but each particle will retain its atmoſphere as before.

THE reaſon of this likewiſe is obvious; the attraction of A for fire being double that of B, and their repulſive powers will be different.

CASE X. The ſame things being ſuppoſed, the heat of B will be equal to that of A, notwithſtanding it contains but half the quantity of fire.

FOR theſe are the proportions which they would retain at the common temperature, or when placed near each other, as above: and this rule determines the heat, as is evident by what follows.

CASE XI. The ſame being ſuppoſed, an equal quantity of fire added to A and B, will heat B twice as much as A; and the quantity of fire neceſſary to raiſe them to equal heats, will be in proportion to the quantities of fire which, at the common temperature, they naturally retain. The like may be obſerved with regard to cold.

FOR if to A a third particle were applied, which had but half the heat, but which would naturally retain as much fire as A, it would take one fourth of A's fire from it, by caſe VI.

BUT if the ſame particle were applied to B, it would take away half of B's fire to raiſe it to the above heat, though one third only would render their quantities as 2 to 1; as is obvious by what was ſaid in the IX. and X. caſes.

THE mixture of bodies, which at the common temperature retain different proportions of fire, when theſe bodies are heated at different degrees, and the phenomena reſulting from them, as alſo the equal affection of the ſenſe, and of the thermometer by thoſe different bodies when at like temperatures, may be underſtood from this caſe, and thoſe which precede it.

DEFINITION I. When, at the common temperature, a particle is made to retain a greater quantity of fire than it would naturally do in that temperature, the particle ſhall be hot.

Def. II. And if it be made to retain leſs fire than it naturally would do in that temperature, it ſhall be cold.

Bodies are expanded by heat merely becauſe their particles are ſurrounded with greater atmoſpheres of fire, and therefore repel each other, ſo that they are kept at a greater diſtance than before. Cold is cauſed in the ſenſe merely by diminiſhing, and heat merely by increaſing the quantity of fire in the part, and therefore cauſing a like contraction or expanſion of that part *. I do not therefore ſee any reaſon for ſuppoſing either that the particles of bodies are in a ſtate of vibration when hot, or that the particles of fire themſelves are in that continual rapid motion which others imagine †. If the latter was the caſe, the particles of air ought to be exceedingly hot, by reaſon of the great and therefore condenſed atmoſpheres of fire which they contain ; but at the common

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* It muſt be obſerved that pain, which accompanies theſe ſenſations when violent, is not to be confounded with the ſenſations themſelves.

† Vide Macquer's Chymical Dictionary.

temperature they are no hotter than others which retain atmoſpheres much leſs.

Case XII. Imagine a particle attracting fire, and another which would diminiſh that attraction; if they are at a ſufficient diſtance from each other, the latter will not affect the attraction of the former for fire, but it will diminiſh that attraction more on being brought nearer; and when they meet, the diminution will be greateſt of all.

Case XIII. The ſame things ſuppoſed, the attraction of the former particle for fire will be leſs, according as it is already combined with more of the latter.

Case XIV. The attraction of the former particle for thoſe of the latter kind will be leſs, according as it is already more ſaturated with them; for they will exiſt in the atmoſphere at a greater diſtance from the particle; and therefore they will alſo have leſs power of diminiſhing the particles attraction for fire *.

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* Vide Caſe XII.

THESE caſes were ſuggeſted to me by the following experiments.

EXP. I. It is well known that if oil of vitriol and water be combined, a great degree of heat is generated.

IN a ſmall ſlender phial I put water, and added to it about an equal bulk of oil of vitriol; the acid was poured down the ſides of the phial, and remained at the bottom; as ſoon as this was done, and before ſhaking them together, I marked the height of the liquid on the outſide, and then well mixed the ingredients. A very great heat preſently ſucceeded, and afterwards I found that the ſurface of the liquid was below the mark. But it could not have evaporated, becauſe it was cloſe ſtopped with a cork *.

EXP. II. I put ſome pounded ice into a phial,

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* I have ſomewhere read that a drop of concentrated vitriolic acid, and another of water being put into a ſlender tube, penetrated each others dimenſions, ſo as to be leſs in bulk. But the ſpecific gravity of oil of vitriol firſt led me to try the above experiment.

and added ſpirit of nitre highly concentrated till it juſt covered the ice; I immediately marked the height of the liquid; and after the ice was diſſolved, found, contrary to what happened with the above mixture, that the liquid had riſen above the mark. A great degree of cold was generated by the combination.

I DO not remember that there was any inaccuracy in making theſe experiments; and they ſeem to indicate that the heat and cold in theſe mixtures are connected with the contraction and expanſion of the compound. I at firſt thought that it was from the contraction or expanſion of the body as a whole; but on conſidering that ice is more expanded than the water from which it was formed, and yet that heat is generated by the congelation *, I concluded that the contraction and expanſion muſt be conſidered as in the particles themſelves; and that this always takes place in the particles on theſe occaſions, though particular circumſtances (ſuch as new arrangements of the particles, and the like) may in ſome caſes hinder the rule from obtaining in the whole maſs or body.

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* Dr. Black.

THE particles of the oil of vitriol and water therefore were, by ſome very powerful agent, drawn nearer to each other, ſo as to occupy leſs ſpace than before; and as the principle of cohesion in earth, or common gravitating matter, was ſhewn to be phlogiſton, it ſeems to have been effected by the agency of that principle. As the phial was corked, it did not ſeem likely that any freſh particles of phlogiſton ſhould have been derived from without. Beſides, if that had been the caſe, the weight of the compound would have been leſſened: but by repeating the experiment, and weighing the ingredients before mixture, and again immediately after, and ſuffering the whole to remain in the ſcale, properly ſuſpended, till cold, I did not find this to happen. Now phlogiſton muſt combine more firmly with the bodies according as they are already leſs ſaturated therewith * That water contains this principle in conſiderable

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* Imagine a particle of earth, and that phlogiſton be added to it in the manner of an atmoſphere. The particles of phlogiſton at the greateſt diſtance, being leſs attracted, will retain greater atmoſpheres of æther. Phlogiſton may be added to the particle till it can retain no more, by reaſon of the repulſion

conſiderable quantity is obvious, by its affording nouriſhment to vegetables, by its being a conductor of electricity, and alſo by an experiment of Dr. Prieſtley, in which the calx of mercury was reduced by the phlogiſton from that fluid, and this alſo ſhews that it was not contained in a ſtrongly combined ſtate. The oil of vitriol, by reaſon of the vitriolic acid *, is of a more pure earthy nature, or is leſs phlogiſticated; and therefore its attraction for phlogiſton will be greater than that of water. The phlogiſton of the water will therefore be laid hold of by the acid, and that ſtill retaining the water, a cloſe and intimate connection, or ſtrong attraction, or coheſion will take place between the particles of the acid and thoſe of the water, ſo that they will be drawn into leſſer dimenſions. By this more intimate combination the

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pulſion of the ætherial atmoſpheres. For the ſame reaſon two ſuch particles, when overcharged with phlogiſton, will have their coheſion diminiſhed by freſh addition inſtead of increaſed. A particle thus overcharged, and another charged leſs, will cohere more ſtrongly than the two particles juſt mentioned, for the outer phlogiſton of the former particle will be more forcibly attracted by the latter, than by any homogeneous one, and therefore will more firmly combine with it.

* Vide ſection VI.

particles of phlogiſton will loſe part of their attraction for æther; and, for the ſame reaſon, the earthy particles of the acid applied, will loſe part of their attraction for fire. *The fire which thus becomes ſuperabundant is, I take it, the cauſe of the heat of the mixture.*

If we imagine the oil of vitriol to be again ſeparated from the water, a degree of cold will be generated, equal to the heat from their mixture, becauſe the attraction of the earthy particles for fire will be reſtored.

This, and what follows to caſe XVII. will probably explain the cauſes of heat and cold ariſing from chymical mixtures in general.

Case XV. It ſeems therefore that "whenever heat is generated, without any addition of freſh phlogiſton, it argues an increaſe of attraction between the particles of phlogiſton and earth, and a conſequent diminution of the attraction between the particles of earth and fire." The contrary may be obſerved with reſpect to cold.

When Bodies return from an elaſtic to a fluid ſtate, or from a fluid to a ſolid ſtate, heat is generated; and cold in the contrary caſes, as Dr. Black and others have ſhewn; the above may be the reaſon. And there are other phenomena of the kind which will occur to the Reader, probably depending on the ſame principle.

Case XVI. When a particle of phlogiſton is combined with a particle of earth, heat is generated, if the foregoing reaſoning be true; and if the combination be rendered ſtill more intimate, freſh heat will ariſe.

Case XVII. It follows therefore, that if phlogiſton weakly combined with one particle, be transferred from thence to another, with which it may form a ſtronger, or more intimate combination; the heat generated by the combination, in the latter caſe, will be greater than the cold generated by the decompoſition in the former: and this difference will be greater, according to the difference of the two attractions or combinations. The contrary may be obſerved of the generation of cold; and the reaſons are obvious from what has been ſaid.

THIS caſe may obtain in ſome kinds of chymical mixtures, particularly in certain ſolutions of metals by acids. &c. But the former part of it ſeems to me to be the principle on which the heat in combuſtion depends.

THAT phlogiſton has different degrees of attraction or forces of combination, with different particles, and with the ſame particle * in different circumſtances, appears by the following

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* Vide caſe XII. &c. It may be obſerved that the attractive forces of ſpherical bodies decreaſe in the duplicate ratio of the diſtance. The rarity of the phlogiſton therefore muſt increaſe in the direct proportion of the diſtance, as happens with our atmoſphere, and with the atmoſpheres of fire about particles of earth, as ſhewn before. Sir Iſaac Newton has ſhewn, in the 369 page of his Optics, 3d edition, that the attraction of coheſion is as the diſtance; which anſwers to the above. Particles of earth do not ſeem to exert their whole attractive force on phlogiſton; but after ſaturation with it they ſeem to have ſome attraction left for fire, as water ſaturated with one ſalt can yet attract another: hence the cohering forces of bodies reach but to a given diſtance beyond them. The like may perhaps be the caſe with phlogiſton.

lowing conſiderations. After phoſphorus is burnt, if the acid be urged with a great heat, it will give evident ſigns of containing phlogiſton, as Mr. Macquer obſerves. Lead eaſily parts with a certain portion of its phlogiſton by calcination; but retains another part very obſtinately. Phlogiſton with pure vitriolic acid, forms ſulphur; but if water be previouſly combined with the acid, it forms an incombuſtible liquid. Light cannot form a combination directly with air; but if it be previouſly combined with proper matters, it is then combuſtible phlogiſton. Phlogiſton transferred from charcoal to zinc is much leſs eaſily combuſtible than while it was in the charcoal, and many other inſtances might be produced.

PARTICLES of greater fixity or force of coheſion (I mean homogeneous ones, and in certain circumſtances) appear to have leſs atmoſpheres of fire than thoſe homogeneous ones whoſe fixity, or force of coheſion, among one another is leſs. Hence we find that the particles of water, for example, have atmoſpheres of fire ſo ſmall that with the uſual heat of the air, their ſpheres of repulſion do not reach beyond their ſpheres of coheſion *. For in that heat they will coaleſce after having been rendered elaſtic by a greater degree; and the particles of many bodies coaleſce in an heat much greater. On the contrary, air, which is a vaſt deal leſs fixed, or the coheſion of whoſe particles one with another is a vaſt deal leſs, is very elaſtic with the uſual heat of the atmoſphere, and even in the greateſt cold we have been able to produce. The atmoſpheres of fire therefore extend to a greater diſtance beyond the cohering ſpheres of the particles, and a prodigious quantity of fire ſeems to be contained in the atmoſphere of a particle of air, in proportion to what is retained by one of the fixed particles juſt mentioned, inſomuch that air ſeems to be the great reſervoir of this principle. If, therefore, we ſuppoſe the cohering ſpheres of particles to be compoſed of phlogiſton, and as phlogiſton ſeems, by what has been ſaid, to weaken the attraction of particles for fire, we have a reaſon why fixed or cohering particles have leſs atmoſpheres of fire than thoſe which are leſs ſo: homogeneous particles, however, muſt be underſtood in theſe caſes; for

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* See alſo ſection IX.

with heterogeneous ones the caſe will be different *. Alſo, even homogeneous particles overcharged with phlogiſton will not cohere ſo well as when they have leſs, as appears from what has been ſaid; and yet their attraction for fire will be leſs.

It ſeems to me that the fixed or unalterable particles of bodies, as of air, water, &c. † are not pure gravitating matter, or earth, but particles of earth and phlogiſton combined together, in different proportions and manners, ſo as that they may have, originally, greater or leſs attractions for phlogiſton and fire, and repulſion for æther. What was advanced in caſe XII. &c. will hold good with any of theſe, but with different degrees of force, and the idea, properly purſued, might have its uſes. But however this be, the fact, that *moveable phlogiſton*

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* Thus air will cohere ſtrongly with more fixed particles (as in nitre), though its particles will not cohere among themſelves: the reaſon is plain from what was ſaid in caſe XIV.

† I do not mean the primary or abſolutely ſolid particles, but theſe on which the invariable ſecondary properties of bodies depend, and which experience ſhews to be indeſtructible. Thus water is the ſame in all ages.

ton * *is more intimately combined in ſome bodies than in others, and that it weakens the attraction for fire in proportion to the force of that combination*, ſeems probable from what has been ſaid, and will appear ſtill more probable when we apply it to the ſolution of the phenomena of combuſtion in the next ſection. As I do not pretend to demonſtration, and as theſe caſes contain as much as is judged neceſſary to the ſubject which they were intended to elucidate, I ſhall not here purſue the idea any farther.

WHAT has been ſaid of the attraction of fire by particles of earth in the foregoing caſes, may be applied, under proper reſtrictions, to the repulſion of æther by the ſame particles, and to the attraction of it by particles of phlogiſton, as is evident from the preceding diſcourſe, and therefore I need not enlarge on it.

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* I mean phlogiſton which may be transferred from one body to another.

SECTION VIII.

Of the Origin of Heat in Combuſtion.

HAVING in the preceding ſection given ſome conjectures concerning the manner in which fire is retained by the particles of bodies, and by what laws it is regulated; we may proceed to examine how far they agree with the heat which attends combuſtion.

IT was ſhewn in the third ſection that combuſtion is a truly chymical proceſs, and depends on this principle, "that the air attracts phlogiſton from the combuſtible body by means of a ſuperior affinity."

IN the fourth ſection it appeared that when a particle of air attracted phlogiſton from a particle of a combuſtible body, heat was generated, and it was promiſed that the cauſe of that heat ſhould be conſidered in a future ſection.

It has appeared probable, in the courſe of the laſt ſection, that the particles of bodies have an attraction for fire; and that the attraction is greater, as the particles are freer from phlogiſton. It further appeared, that according as the ſame quantity of phlogiſton is more intimately combined, it cauſeth a greater diminution of the particles' attraction for fire. It alſo appeared that the particles of air have very great attractions for fire, and thereby retain vaſt atmoſpheres of it; but that the quantity of fire retained by fixed homogeneous particles is on the contrary very little.

Now, agreeable to the ſeventeenth caſe in the preceding ſection, imagine a particle of the phoſphoric acid ſaturated with phlogiſton, and that it be applied to a particle of air: let the attraction of the acid for phlogiſton be weak, but that of air ſtrong, the particle of the air will attract the phlogiſton from the acid; and the combination will be ſtronger than with the acid in proportion to the difference of the attractions; the heat generated by the combination, will be greater than the cold generated by the decompoſition, for the ſame reaſons: in other words, the ſuperabundant fire of the air will be more than ſufficient to ſatiate the increaſed attraction of the acid, by the exceſs of the attraction of the air above that of the acid. And this ſeems to me to be the manner in which heat in combuſtion is generated.

COROLLARY I. The difference of the attractions may be eſtimated, by meaſuring the generated heat, and attending to the following circumſtance.

COR. II. The ſuperabundant fire of the air will be attracted by, or gravitate towards, the bodies around, in order to reſtore the equilibrium. And according as theſe ſurrounding bodies, or the extraneous particles, have leſs attractions for fire, the more will they be heated by this ſuperabundant fire. Vide caſe XI.

COR. III. Suppoſing a number or maſs of the phoſphoric particles, the more of them that are decompoſed in a given time, the greater will be the quantity of ſuperabundant fire generated in that time; and therefore, ſuppoſing the ſurrounding bodies, or extraneous particles to be the ſame, the hotter will they be: but if they be different, their heat will be different according to their attractions for fire, as may be collected from the eleventh caſe in the laſt ſection.

Now that fire is taken from the air in combuſtion appears from hence, that its elaſticity, or repulſive power is weakened, and its dimenſions decreaſed, ſo that the particles are not kept at ſo great a diſtance from each other as before; for it was ſhewn in the firſt five caſes that the particles are kept at a diſtance from each other by means of their atmoſpheres of fire. Mr. Cavendiſh has ſhewn, if I remember right, that the ſpecific gravity of fixable air is to that of common air as 1½ to 1, and yet the abſolute gravity of the fixed air muſt have been greatly diminiſhed by its combination with phlogiſton, ſo that the difference of elaſticity of pure fixable and common air muſt be ſtill greater than in that proportion*. Thoſe who have conveniencies for making experiments would do well to examine and aſcertain theſe matters.

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* See alſo the next ſection.

other words, the ſuperabundant fire of the air will be more than ſufficient to ſatiate the increaſed attraction of the acid, by the exceſs of the attraction of the air above that of the acid. And this ſeems to me to be the manner in which heat in combuſtion is generated.

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Now that fire is taken from the air in combuſtion appears from hence, that its elaſticity, or repulſive power is weakened, and its dimenſions decreaſed, ſo that the particles are not kept at ſo great a diſtance from each other as before; for it was ſhewn in the firſt five caſes that the particles are kept at a diſtance from each other by means of their atmoſpheres of fire. Mr. Cavendiſh has ſhewn, if I remember right, that the ſpecific gravity of fixable air is to that of common air as 1½ to 1, and yet the abſolute gravity of the fixed air muſt have been greatly diminiſhed by its combination with phlogiſton, ſo that the difference of elaſticity of pure fixable and common air muſt be ſtill greater than in that proportion*. Thoſe who have conveniencies for making experiments would do well to examine and aſcertain theſe matters.

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* See alſo the next ſection.

If we take a ſurvey of the different inflammable bodies we ſhall find that ſome of them require a greater heat to kindle them, and others a leſs. And therefore before a body can be enabled to continue its own combuſtion, ſuch a quantity of its particles muſt be decompoſed in a given time, as will be ſufficient to furniſh a due portion of ſuperabundant fire. But, if the ſubſtance be perfectly and uniformly inflammable, a ſufficient number of particles thus once decompoſed, will furniſh fire enough to equal the degree of extraneous heat firſt applied, and therefore to continue the generation of a like quantity ſucceſſively, by means of new decompoſitions, as long as any of the ſubſtance remains. Vide ſection VI.

Supposing an equal quantity of ſuperabundant fire generated by different bodies in the ſame time, "the heat of the flame will be greateſt in thoſe whoſe phlogiſticated particles, after parting with their phlogiſton, have the leaſt attraction for fire;—— which contain the feweſt extraneous particles——and whoſe extraneous particles attract fire leaſt."——

——The flame of ſpirit of wine, when ſufficiently gentle, is not even ignited, as will be ſhewn,

ſhewn. For this liquid contains ſo large a proportion of extraneous particles *, and theſe ſeem to have ſo great an attraction for fire, that the ſuperabundant fire is not ſufficient to make them red hot. Perhaps there is not more fire ſeparated from the air in a given time by oil, than by ſpirit of wine: but as oil contains a much leſs proportion of extraneous particles, the ſame quantity of fire is ſufficient to ignite the vapour of oil, though it cannot that of ſpirit of wine.

If we ſuppoſe all other circumſtances alike, the heat of the flame will be greateſt in thoſe bodies which, in a given time, ſaturate the greateſt quantity of air, or generate the greateſt quantity of ſuperabundant fire. Perhaps lamp-oil has as great a proportion of phlogiſton as zinc; yet the flame of zinc, if I am rightly informed, is by much the hotteſt, and therefore the decompoſition in the metal proceeds more rapidly than in the oil.

I used formerly to think that thoſe bodies would require the greateſt heat to begin

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* Water.

their combuſtion, which attracted the phlogiſton moſt ſtrongly; heat weakening the attraction between phlogiſton and earth. But that this is not always the caſe appears from hence, that if this rule held good, thoſe different bodies would ſuffer equal decompoſitions in equal times, which does not agree with the laſt paragraph. In chymiſtry alſo, we find that the phlogiſton may be transferred from the earth of charcoal, to the calx of zinc; from thence to the phoſphoric acid; from the phoſphoric acid to the vitriolic; and from thence again to the nitrous acid, or air. Yet charcoal burns with more difficulty than ſulphur; and zinc does not begin to flame but with an heat vaſtly ſuperior to that which kindles phoſphorus. Chymiſtry, however, furniſhes us with ſomething analagous to this: thus all the acids, except the phoſphoric, attract fixed alcali preferably to calcareous earth; but that acid attracts the earth preferably to the alcali. The vitriolic acid attracts fixed alcali more than the nitrous; but the nitrous acid attracts phlogiſton ſtronger than the vitriolic. But one reaſon of the greater difficulty in inflaming zinc than phoſphorus * may be, that the former contains extraneous particles which ſhield and defend the phlogiſton from the action of the air: thus water defends phlogiſton from the action of the vitriolic acid, and ſulphur cannot be formed till by heat, or otherwiſe, the water is diſſipated †. For that the difficulty of the combuſtion of zinc does not proceed from its fixity, or difficulty of being raiſed into vapour alone, appears from hence, that the combuſtion of lead is effected without ſuch vapour, or merely by calcination. But when, by the action of heat, the phlogiſton of the zinc is rendered combinable with air, the decompoſition proceeds rapidly indeed! by reaſon of the weak attraction of the phlogiſton for the calx, or of the great eaſe with which the air now attracts the phlogiſton from that body.

The heat attending nitrous combuſtions may be underſtood by referring to what was ſaid concerning them in the ſixth ſection. A certain degree of extraneous heat muſt be applied

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[*] See alſo caſe vii. ſection VIII.

[†] Fixable, and phlogiſticated air may, for a like reaſon, be analogous to ſulphur, and the volatile vitriolic acid.

plied in order to enable the phlogiſton and air of the nitre to combine, and fire is ſeparated from the air by the phlogiſton, in the ſame manner as from the air of the atmoſphere in common combuſtion.

Before oils can be ſet on fire by the nitrous acid a ſufficient degree of heat muſt be generated by their *chymical mixture*[*] in order to enable the phlogiſton and air to unite; and then the combuſtion and conſequent exploſion happen as in ordinary nitrous combuſtion.

The generation of fixable air is a conſequence of combuſtion; and as this is generated in fermentation, in reſpiration, and certain other proceſſes [†], there muſt have been a combuſtion. Theſe combuſtions take place in the ordinary heat of the air, and are, properly ſpeaking, *ſpontaneous calcinations*. The heat is leſs than in inflammation, becauſe of the number of extraneous particles among which it is ſhared: and the light of combuſtion * is not viſible by reaſon that it is ſtifled by theſe particles, and alſo becauſe it is too rare. Thus fixed air may be produced by heating ſulphur in a cloſe veſſel; and by repeating the proceſs, the whole ſulphur might, I imagine, in time, be decompoſed as effectually as by inflammation, and yet by reaſon of its rarity, the light ſhall not be viſible. Thus alſo coal, iron, liver of ſulphur, and other ſubſtances expoſed to air loſe their phlogiſton in time; and yet by reaſon of the rarity of the light, and the ſlowneſs with which the decompoſition proceeds, neither light nor heat are ſenſible.

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[*] Hence the uſe of oil of vitriol. The like reaſoning may be applied to the combuſtion of pyrophyrus by common air.

[†] This is to be underſtood in caſes where fixed air is actually generated, not where it pre-exiſted and was only expelled as in effervefcent mixtures, &c.

By conſidering the degree of affinity which phlogiſton has for the particles of bodies with which it is combined, the volatility or fixity of theſe particles, and the nature and quantity of the extraneous particles which enter into the compoſition of different bodies, we may have the reaſons why ſome bodies inflame with leſs heat than others; why they burn more or leſs rapidly; why the combuſtion of ſome bodies cannot be effected without a continual

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* Vide ſection IX.

application of extraneous heat, and the like. Hence alſo the difference between inflammation and incineration or calcination. The other phenomena of the ſecond ſection will eaſily be underſtood from hence, and from what has been already ſaid.

WHETHER the hypotheſis on which I have proceeded be true or falſe, experience muſt determine; thoſe who have leiſure and convenience would do well to proſecute the Inquiry. The Reader, on conſidering the importance of the ſubject *, will excuſe me for detaining him ſo long with conjectures; and the next ſection, I hope, will make him ſome amends. I have only been able to gueſs at the theory of the heat of combuſtion; that of the light, at leaſt the obſervational part of it, I think I can venture to offer as certainty.

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* Vide ſections X. XI. and XII.

SECTION IX.

Of the Light and Colours which ariſe on the Ignition, and Combuſtion of Bodies.

SOME ingenious philoſophers have of late found that the calxes of certain metals may be reduced by means of *light;* and that other phlogiſtic proceſſes may be performed by it: they have therefore imagined, that light is *phlogiſton.* The following conſiderations will perhaps in ſome meaſure clear up this matter.

OBSERVATION I. When an incombuſtible body is heated to a certain degree it emits light; and the light increaſes as the heat becomes greater.

OBS. II. The light which is firſt emitted is of a reddiſh colour, ſo much ſo indeed that the body is ſaid to be *red-hot.*

Obs. III. As the heat increaſes, the colour verges more towards orange and yellow diluted with white; and when the heat is very intenſe, the colour becomes ſo white that the body is ſaid to be *white-hot.*

Obs. IV. But if the heat be ever ſo much increaſed; yet, if there be no combuſtion, the colour is never found to vary from the white towards blue, purple, or violet.

It is known that light conſiſts of rays variouſly refrangible, and that this ariſeth from the different ſizes of their particles, theſe rays being moſt eaſily refracted whoſe particles are the ſmalleſt; when theſe various particles of light are combined with a body, the leſſer ones will be attracted and held moſt powerfully, and the larger ones leaſt, for the ſame reaſons that the rays compoſed of them are differently refracted by that body. When air is combined with a ſubſtance, the application of a proper degree of heat will ſeparate it therefrom, and cauſe it to fly off in its elaſtic ſtate. In like manner, when the particles of light are combined with a body, and heat be applied, theſe particles will be diſlodged, and expelled from the body by the action of the fire, and as they have a *polar* virtue (as appears by the double refraction of iſland chryſtal) they will take the rectilinear diſpoſition, and conſtitute *rays of light*. But be this as it may, thoſe particles which are largeſt, and which therefore are leſs forcibly retained by the body, will begin to be diſlodged with a leſs degree of heat than the ſmall ones, which the body retains more powerfully; and as theſe are the particles which conſtitute the red-making rays, the body muſt appear red; this red, however, will not be perfect, becauſe ſome of the other particles alſo will be expelled, though in leſs quantity than is ſufficient to compoſe a white: as the heat increaſes, the orange, yellow, and other particles will be expelled in more equal proportion, and therefore the colour will verge from the red towards white, ſo that when the heat becomes ſufficiently intenſe, they will all be expelled alike, and the body appear perfectly white. Thus in diſtillation, when liquids of different volatility are contained in the alembic, if the fire be gentle, the moſt volatile will come over more pure; and its purity will be leſs as the heat is more augmented.

But in thoſe bodies which are combuſtible, we are preſented with very different phenomena; if copper, for example, be heated, it will firſt ſhine with a red heat, which will afterwards be whiter according as the heat is increaſed, as ſhewn above; but if it be made to flame, the colour emitted will be green. Zinc may, in like manner, be heated red-hot, and the red colour will become whiter as the heat increaſes. But if it be made to flame, the colour is intenſely white. The flame of ſulphur is blue, of tallow, yellow, of lamp-oil, orange; and there are hardly two bodies whoſe flames are exactly alike. Incombuſtible bodies, therefore, when ignited, and alſo thoſe combuſtible ones which ignite before they flame, emit light in the following order; red, orange, yellow, green, blue, indigo, violet. But this is not the caſe in combuſtion.

Obs. V. The ſubſtance which burns with the leaſt heat of any that we know is phoſphorus;

rus; the colour of its weak flame is a violet-blue, if I am rightly informed. Sulphur burns with an heat leſs than that of ignition, and its colour (eſpecially when the flame is gentle) is blue; alcohol burns with an heat greater than ſulphur, yet below that of ignition, if properly managed, and the colour emitted is blue, though leſs ſo than ſulphur.

From hence it appears, that as in ignition the red-making rays are moſt copiouſly emitted (and at firſt almoſt entirely); thoſe which are emitted moſt copiouſly in combuſtion are, on the contrary, the violet. To account for this difference, the Reader is requeſted to attend to the following reaſoning.

It was ſhewn in the foregoing ſections, that heat is neceſſary to enable the air to attract the phlogiſton from bodies; and that the fire ſeparated from the air by the phlogiſton, ſerves afterwards inſtead of extraneous fire to keep up the heat of the body, and enable the air to continue the decompoſition. The fire expelled is ſo copious as even to heat the body more than is ſufficient to enable the air to attract the phlogiſton, as is evident by the flame of a combuſtible body ſetting fire to a body which requires a greater heat than it to begin the combuſtion; the intenſeneſs of the heat, therefore, and the violent attraction of the air, will diſlodge the phlogiſton from the body faſter than the air (eſpecially when it begins to be ſatiated) can readily combine with it; and thoſe particles which are not immediately combined, attract large atmoſpheres of æther, which render them incapable of combination with air, and therefore they are driven off in the form of *light*.

Now, as in ignition, bodies retain the blue light moſt powerfully, and part chiefly with the red, and other particles which compoſe the leſs refrangible rays; ſo in combuſtion, the air moſt eaſily diſengages and attracts the larger particles, with which being firſt nigh ſaturated, the ſmaller ones remain behind as the ſuperabundant particles above ſpoken of; and which, by acquiring ætherial atmoſpheres, become particles (and are driven off in the form) of light.

The flame therefore muſt appear of a colour on the violet ſide of white, as we find to be the caſe *.

Bodies which ſhine by ignition can, for reaſons juſt given, advance in colour only from red, through a dilute orange and yellow, to white, and can never paſs from that white to green, blue, and violet; ſo neither can the light of combuſtion paſs on to yellow, orange, or red; but yet we find that ſome flames are tinctured with theſe colours. Thus the flame of a candle is yellow, that of an oil-lamp orange, and of wood red.

But it muſt be obſerved that the above rule holds good only in thoſe flames whoſe heat is below ignition. When the heat is intenſe, the particles which compoſe the vapour

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* The ſmaller particles will alſo attract æther faſter than the large ones. The reaſon that light does not thus appear when the vitriolic acid &c. takes phlogiſton from bodies ſeems to be, that by reaſon that the particles have leſs atmoſpheres of fire, they are nearer in contact with the phlogiſton, and therefore by attracting it prevents its eſcape; or the light expelled may be too rare to be viſible for the ſame reaſon.

are ignited, and the light which proceeds from them is mixed with the light of the combuſtion; but the light of the ignited vapour is emitted in a contrary order to that of the combuſtion, the latter beginning at violet, the former at red *; and therefore the mixt colour of the flame will be varied according to the degree of heat, the proportion of the light of the combuſtion to that of the ignition (which in ſome caſes will depend on the nature and proportion of extraneous particles † in the vapour), and to other circumſtances. Thus, the flame of wood ſeems to be in the firſt degree of ignition, or *red-hot*, and the proportion of the light, to that of the combuſtion, is ſo great

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[*] As a farther proof of the difference obſerved, thoſe flames which are ignited are opaque; but thoſe that are not ignited are tranſparent. The flames of oil, and of alcohol, properly managed, will ſhew this to advantage.

[†] How theſe affect the heat of the flame (on which its ignition depends) may be gathered perhaps from the laſt ſection; and as the ignition is leſs, its light is redder, as was ſhewn above. Different bodies may alſo, perhaps, have different proportions of the blue light in their compoſition. Some flames ſeem likewiſe not to have any, or but very few, extraneous particles, and therefore are ſtill blue, though their heat be great.

that its colour is predominant; the flame of oil, though leſs red, does not ſeem to be much hotter than that of wood, but the light of the combuſtion is in greater proportion, ſo as to dilute the red to an orange. The ſame may be obſerved of the flame of a candle which is of a dilute yellow; and in the like view the colours of flames of other bodies may be conſidered, ſome of which I have arranged in the following table.

TABLE OF THE COLOURS OF FLAMES.

Bodies arranged according to the degrees of heat neceſſary to begin their combuſtion.	Light of the combuſtion.	Light of the Ignition.	Proportion of the lights of C, and I.	Colour of the flame.
The weak flame of phoſphorus	Violet-Blue	None	All C	Violet-Blue
Sulphur	Blue	None	All C	Blue
Alcohol	Greener Blue	None	All C	Dit. a little greener
Small wood	Ditto ?	Red-white	I, moſt	Reddiſh white
A pitch torch	Ditto ?	Ditto ?	Ditto	Ditto
Lamp-oil	Ditto ?	Ditto ?	Ditto, leſs	Orange-white
Tallow	Ditto	Ditto ?	Ditto, ſtill leſs	Yellow-white
Camphire	Ditto	Ditto ?	Ditto, ſtill leſs	Ditto more white
Nitre and coal	Ditto ?	Yellow-white	I, moſt ?	Ditto ſtill more
Copper	Ditto ?	Ditto ?	C, moſt ?	Green-white
Iron	Ditto ?	Whitiſh	I, moſt	White
Zinc	Ditto ?	Whitiſh	Ditto, leſs	Whiter

Others might have been added; but even thoſe which I have given are very incorrect, being ſet down only by gueſs; and the table is offered merely as a ſketch of the ſubject, to be proſecuted by thoſe who have proper inſtruments, and other conveniencies.

It is to be noted, however, that different parts of the ſame flame are unlike in colour: thus, the bottom of the flame of a candle or oil-lamp is blue; it grows leſs blue by degrees till it ends in a yellowiſh white; but this white, towards the top, verges towards orange, and ſtill further, towards red (eſpecially when the flame is advantageouſly diſpoſed), till it ends in unignited vapour or ſmoke.

To underſtand the reaſon of this it muſt be obſerved, that at the bottom, where the decompoſition begins, the light emitted is only that of the *combuſtion;* for it takes up ſome little time to ignite the particles; and therefore they do not begin to emit the light of ignition till they have aſcended ſome way up in the vapour. But when that takes place, the blue colour of the combuſtion begins to be changed, till at laſt the mixed colour in the middle is of a yellowiſh white. But the light of the combuſtion being leſs and leſs towards the top, till perhaps it quite ceaſes, and the ſurrounding air cooling the ignited particles into a leſs white heat *, the colour of the flame towards that part is more red, till at laſt the particles loſe their ſhining heat, and paſs off in the form of unignited vapour. When the wick is long, and alſo ignited, the latter phenomena are more conſpicuous; for the red light of the wick, and of the particles that eſcape from it, being mixed with that of the flame, tinctures it, eſpecially at the point, more highly with red. In daylight, or ſun-ſhine, the latter phenomena appear to ſtill greater advantage, the weak light of the combuſtion being then leſs capable of interrupting that of the ignition.

THE colour of the light of the combuſtion of bodies may be known by obſerving the bottom, where it is as yet unaltered by that of the ignition, for reaſons given above. Thus, if you faſten a piece of camphire on a wire, and inflame it, holding it up in the air, you will ſee

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* Hence the Conic form of the flame.

a blue light at the bottom. The light of the combuſtion in all bodies muſt be more or leſs blue, becauſe mixtures of the moſt refrangible rays produce only various ſhades of that colour *.

In the above table only the middle part of the flame is conſidered, where the light is compound, as in tallow ; and only in the weakeſt ſtate, where it is ſimple, as in alcohol. But if a quantity of alcohol be burnt, ſo that the flame riſes high, the particles will be ignited. And if we examine the upper part of the flame, and compare the colour with what has been ſaid, we ſhall find this to be the caſe. The other phenomena of the lights of ignition, and combuſtion, either ſeparate, or conjoined, may perhaps be underſtood by proſecuting the principles above laid down.

Corollary I. Bodies retain a conſiderable quantity,

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* Perhaps even the ſtrongeſt ignition that we can cauſe by our fires does not yield a perfectly white heat. The flame of zinc, however, is intenſely white, if I am rightly informed. The light of the ignition is, by that of the combuſtion, diluted to a perfect white.

quantity of particles of light in their pores, or otherwiſe. Theſe particles are diſlodged and expelled from thoſe bodies by a proper degree of heat, and the largeſt particles moſt eaſily, by reaſon that they are leſs forcibly retained. Hence the light of ignition.

Cor. II. Phlogiſton *combined* with bodies cannot be expelled by heat alone, though light can; thus charcoal, heated in a cloſe veſſel, though it may be made to emit light, yet is not found to part with its combined phlogiſton; yet the light of the combuſtion is this very phlogiſton ſet at liberty by the combined action of heat, and the attraction of the air. Vide ſections VII. and IX.

Cor. III. Phlogiſton therefore is light in a ſtate of combination with bodies, forming a conſtituent or eſſential part of them. Light is phlogiſton in an elaſtic ſtate exiſting in their pores. As this laſt is leſs attracted by bodies, they ſhine with a leſs heat: thus electricity and certain phoſphori, ſhine with the uſual heat of the atmoſphere; and ſome of the latter, if expoſed to any particular ſort of the ſun's rays, expel them again in the ſhade; that is, the ſame colour which was forced into the body is afterwards emitted by it, as its attraction for fire, which was diminiſhed by the action of the light, returns.

THAT the light of theſe phoſphori is what I call *the light of ignition* appears by its colour. In ſome caſes of electricity, however, a blue light is obſerved; but this does not happen unleſs a *real combuſtion* takes place, and that this does ſometimes obtain is obvious by an experiment of Dr. Prieſtley, who made fixable air with this fluid. I had drawn up a theory of electricity, and intended that it ſhould have followed this ſection; but found, after I had finiſhed it, that it would ſwell the volume to a much greater ſize than was intended. The electric fluid appeared to me to be *phlogiſton combined with earth, already more intimately combined with a conſiderable portion of that principle;* for air takes it from that earth, as appears by the above experiment; neither can pure phlogiſton combine directly with air, for a reaſon to be met with in the 7th caſe of the VIIth. ſection. That the earth is of this nature appears quantity of particles of light in their pores, or otherwiſe. Theſe particles are diſlodged and expelled from thoſe bodies by a proper degree of heat, and the largeſt particles moſt eaſily, by reaſon that they are leſs forcibly retained. Hence the light of ignition.

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In combuſtion, and ſome other chymical mixtures, perhaps a ſmall part of the heat may be occaſioned by the friction or percuſſion of ſome of the particles, though much leſs than is at preſent believed: but bodies are probably heated by light *, and by electricity, in great meaſure by this means; and entirely by it and the communication of phlogiſton, except in caſes where actual combuſtion is cauſed.

By the ſecond corollary it appears, that phlogiſton, when diſengaged from bodies with which it was combined in a coherent ſtate, aſſumes the form of light; and it was ſhewn before, that fire when diſengaged is the cauſe of heat. I had formerly run the analogy between theſe principles farther, and imagined that fire did not only exiſt in bodies after the manner already deſcribed, but that it alſo

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* Hence opaque and denſe bodies are moſt heated by light.

combined with them in a coherent form, was diſengaged by phlogiſton, &c. and then aſſumed its elaſtic ſtate: and alſo, that it was transferrable from one body to another in a fixed ſtate, in the ſame manner as phlogiſton *. But I could not find means to ſatisfy myſelf of the truth of theſe propoſitions, and the mode of its exiſtence, which I have before ſuppoſed ſeems to agree with the phenomena of heat and cold as exhibited by the ſenſe, and the thermometer; but the truth remains to be cleared up by experiments. In the mean time the hypotheſis that "phlogiſton weakens the attraction of earth for fire, according to the force of their combination," and that "the force of their combination may be intended, or remitted," ſeem ſufficient to account for the phenomena, whatever

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* For example: I argued that cauſticity depended on fixed fire. That a fixed alcali being applied in its mild ſtate to quicklime, the lime combined with the fixed air, and the alcali with the fire. That water expelled fire from the vitriolic acid, quicklime, &c. by means of a ſuperior affinity, as fixed air is expelled from mild alcalis by acids. That ſpirit of vitriol added to cauſtic alcali, the acid joined with the alcali, and the fire with the water, the fire being more than ſufficient to ſaturate the water, &c. &c. The like of air. But theſe things may equally obtain on either ſuppoſition, and what reſpects cauſticity does not ſeem to be true.

be the modes in which fire and phlogiſton exiſt in bodies.

To the mode of its exiſtence which I have ſuppoſed, it may be objected, that if the particles of bodies have the atmoſpheres of fire deſcribed, the bodies which they compoſe ought to repel each other like particles of air; for the atmoſpheres of the particles extending beyond the body, will compoſe a repelling atmoſphere of fire about that body: it may be anſwered, that theſe repelling atmoſpheres are, naturally, balanced by means of electricity. When the latter is removed, the action of the former becomes ſenſible; for two bodies negatively electrified repel each other; or, if the equilibrium be deſtroyed in a contrary way, by the attraction in conſequence of excitation, or by accumulation of electricity, theſe repelling atmoſpheres are equally left at liberty to manifeſt their action; and the electricity may even conſpire with it, if great; hence the mutual repulſion of two bodies electrified poſitively *.

For

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* Electrical repulſion, whether *plus* or *minus*, ſeems to depend on the atmoſpheres of fire, as above. But electrical attraction,

For a reaſon given above, I cannot now enlarge on this ſubject, and therefore ſhall only add, that if the repulſion of the particles of air is not diminiſhed by combuſtion ſo much as might be expected, the cauſe may perhaps partly be diſcovered from hence; and alſo by conſidering that particles of air, &c. may probably be only bodies made up of other particles, theſe again of others, &c.; and that the atmoſphere of a whole particle is made up of theſe portions of the atmoſpheres of its elements which extend beyond the whole particle. Hence the more the particle is condenſed by cold, or by combination with phlogiſton, more fire in proportion will come into the atmoſphere from its pores; the fire ſo expelled will alſo be more expanded. And contrariwiſe when the whole particle is expanded by heat, or the loſs of phlogiſton,

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traction, on the violence with which bodies attract electricity. Thus, a non-electric being properly preſented to a body *minus*, the latter attracts the electricity in the former (and with it the body itſelf) ſo violently as to exceed the mutual repulſion by their atmoſpheres of fire. When the electricity in the two bodies becomes in equilibrio, if the quantities be natural, the attraction and repulſion ceaſe; but if they be ſtill either *plus* or *minus*, the bodies repel each other by means of their atmoſpheres of fire, as before.

ton, the repulſion of the whole particle may depend only on the fire in the atmoſphere thereof. But if we ſuppoſe part of the fire to have been fixed *, the ſolution is perhaps ſtill more eaſy.

When air is applied to zinc, and to moſt other combuſtible bodies, under proper circumſtances, it deprives thoſe bodies of a certain portion of their phlogiſton; but another portion remains behind, of which air cannot deprive them. The fixed particles of bodies, therefore, or what the chymiſts call earth, have ſtronger attractions for phlogiſton than even air, and therefore are *originally* † more pure earth in my ſenſe of the word. The phlogiſton which remains, and which cannot perhaps be taken from them by art, is ſufficient even to keep them coherent. If they were deprived of that extra phlogiſton, they would therefore form a fluid as much more elaſtic than our air, as their attraction for fire would be ſtronger, if the foregoing reaſoning be true.

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* May not the very condenſed fire next to the ſurface of an ultimate particle be ſaid to be *fixed?* Does fire combine with bodies in any other manner than that above deſcribed?

† Vide caſe xvii. ſect. VII.

In phlogiſtic atmoſpheres, the ſmalleſt particles, being moſt attracted, may immediately ſurround the particle; thoſe which are larger next above, and ſo on to the largeſt, which may compoſe the external part of the atmoſphere. Hence may be another reaſon why air in combuſtion firſt, and moſt eaſily attracts the larger particles, as obſerved before. Hence alſo perhaps the reaſon that the ſpheres of coheſion are ſo limited. The like atmoſpheres may be ſuppoſed of the elements of a proximate particle, of the elements again of theſe, &c.; the latter will retain their phlogiſton more powerfully than the former, as being leſs; and their coheſion will, for the ſame reaſon, be ſtronger. This, were it true, might enable us to account for what was obſerved in the laſt paragraph; alſo for the heat generated by oil of vitriol and water, &c. and (together with what was obſerved of the manner in which fire is combined) gives us ſome idea of the internal ſtructure of bodies.

ASTRONOMERS freeze at the thought of the planet Saturn, and entertain a contrary ſentiment with reſpect to Mercury ; but if the proportions of fire, &c. in the different planets be properly adjuſted, as their denſities ſeem to ſhew, it may not be ſo hot in Mercury, nor ſo cold in Saturn as is at preſent believed. Whether the like reaſoning may be applied to the ſun and fixed ſtars ? (See what was ſaid above concerning phoſphori.)

SECTION X.

Of Reſpiration, and Animal Heat.

I FIND by ſome extracts from Dr. Prieſtley's publications, that that great philoſopher has demonſtrated *that the uſe of reſpiration is to carry off the phlogiſton which the blood acquires during its circulation in the body.*

It ſeems to apppear that there is a very cloſe analogy between reſpiration and combuſtion; and this has been an ancient obſervation. Dr. Willis of the laſt century, treating on the heat of the blood, has this paſſage. "Though it ſeems an hard ſaying that *the blood is accended*, yet ſeeing we can attribute its incaleſcence to no other cauſe, why ſhould we not impute it to this? eſpecially ſeeing *the proper paſſions of fire and flame* agree to the *life of the blood.*

"For the chief and moſt eſſential requiſites to continue a flame are theſe three: 1*ſt.* That a free a free and continual acceſs of air be granted to it as ſoon as it is kindled. *2dly*, That it enjoy a conſtant ſulphureous *pabulum* or fewel. And *3dly*, That as well its fuliginous as thicker recrements be continually amanded from it: and ſeeing theſe agree to the *vital flame*, as well as to an elementary, it ſeems very rational to affirm that *life itſelf is a kind of flame*."

THIS learned and very ingenious phyſician ſaw plainly that there was an analogy between combuſtion and reſpiration; and between the heat of flame, and that of the blood. But for want of proper diſcoveries concerning the nature of combuſtion, &c. his ideas were more confuſed and obſcure. A little more light may perhaps be thrown on the ſubject in the courſe of this ſection, and yet ſucceeding authors, who puſh their inquiries farther, will make a ſimilar obſervation on what I have done.

ACCORDING to the diſcovery of the great philoſopher above mentioned, the blood which is brought to the lungs from the body, contains a greater quantity of phlogiſton than that which goes from the lungs into the body. And the air takes this phlogiſton from it in its paſſage through the lungs.

IT appears, by what was ſaid in the Vth. ſection, that when phlogiſton and air combine, heat is generated. And if the conjectures in ſection VII. can be depended on, the heat aroſe from hence, that the phlogiſton by combining with the air weakened its attraction for fire, which therefore gravitated towards, or was attracted by, the bodies around, till an equilibrium, with regard to the attracting powers of the reſpective bodies, again took place. When therefore the particles of air combine with the phlogiſton which they attract from the blood in the lungs, heat will in like manner be generated: that is, the attraction of the particles of air for fire will be diminiſhed as in combuſtion. The blood which is carried to the heart therefore will be hotter than that which is brought to the lungs; and hence one cauſe of the heat of the blood.

BUT when the particles of blood loſt their phlogiſton to the air, their attraction for fire, which by caſe 7th. ſection VIIth. was weakened by the phlogiſton, will, now they are deprived of that principle, be again increaſed. A part of the ſuperabundant fire, therefore, will be attracted by thoſe particles of blood; and they will be carried in this ſtate into the body.

It is known to philologiſts, that if the nerves which ſerve any particular part be deſtroyed, that part will be colder than before, notwithſtanding that the blood circulates through it as uſual. Now as the blood, when it entered that part was already hot, and as, before the nerves were deſtroyed, the heat of the blood was ſupported in its paſſage through that part, and on the contrary, when the nerves were deſtroyed, the blood was cooled in its paſſage through it; it follows *that the heat of the blood is ſupported in its paſſage through a part, by means of the nerves by which it is ſerved.*

The heat in combuſtion ſeemed to ariſe from hence; that when the particles of air combined with phlogiſton, their attractions for fire were diminiſhed: may not the ſame reaſoning be applied to the blood? as the blood in the veins of the body is found to contain more phlogiſton than that in the arteries; and as the heat of the blood in the body appears to depend on the nerves, may we not argue in the manner following?

THE blood in its paſſage from the arteries to the veins has phlogiſton imparted to it either immediately or mediately, by the nerves. But each particle of blood thus combined with phlogiſton will have its attraction for fire leſſened, analogous to what happens to a particle of air in combuſtion. Heat therefore will follow for the ſame reaſon that it follows on the combination of phlogiſton with air in combuſtion, only in a leſs degree. The particles of blood thus phlogiſticated, and rendered unfit for the further purpoſe of cauſing heat, paſs on with the circulation, and freſh ones ſucceed. When the phlogiſticated particles arrive at the lungs, they are decompoſed by the air which attracts their phlogiſton, and from which the particles of blood, in return, take a quantity of fire, ſo that they are again rendered fit for the purpoſe which has been deſcribed. And this ſeems to me to be the manner in which the blood becomes hot.

SECTION XI.

Of the vital and other motions of the Body.

IF ſome purpoſe of the laſt importance to the animal had not been deſigned by *reſpiration*, the all-wiſe Author of nature would, certainly, not have rendered life ſo dependant on that proceſs as to be incapable of exiſting, even a few minutes, without it.

BY the laſt ſection it appeared probable, that the heat of the blood depends on the *nerves;* or that the phlogiſton which the arterial blood acquires in its paſſage to the veins, is communicated to it by thoſe organs.

ALL the vital motion or functions of an animal body are performed by means of the nerves; and all thoſe functions may be reduced to the *contraction of the moving fibres* *. I would ſay, therefore, that *for a nerve to cauſe the action of a fibre, it is neceſſary that the nerve ſhould impart phlogiſton, either immediately or mediately, to the blood flowing through, or by that fibre.*

THAT there is a connection between the action of the fibres, and the phlogiſtication of the blood, appears, I think, by the following conſiderations. I. The heat of the blood depends on the nerves, as appears by the laſt ſection. II. According as more of the voluntary muſcles act, or as their action is ſtronger, more blood is phlogiſticated in a given time; for the heat generated is greater, and the reſpirations are quicker: and III. The motion of the blood through a muſcle is known to be as neceſſary to its action as the nerve; for if the artery be tied, the muſcle becomes paralytic as effectually as if the ligature had been made on the nerve.

THE action of the moving fibres may be divided into voluntary, and involuntary: ſome fibres ſerve for involuntary action alone; others for voluntary; but thoſe muſcles which are for the voluntary motions of the body, are continually exerting involuntary action. The contractions of the arteries, the veins, and other veſſels of the body for the purpoſe of circulating the fluids, &c. are performed by means of moving fibres. The muſcles, membranes, coats of veſſels, &c. are made up of ſuch fibres; there is no ſenſible part of the body but what abounds with them; all theſe are continually exerting involuntary, and moſt of them in walking, voluntary actions, neceſſary to the life and well-being of the animal. Now, as there ſeems to be a mutual dependence between theſe actions, and the phlogiſtication of the blood, as the number of particles of blood is not infinite, but on the contrary, only ſuch a quantity can be admitted into the ſtructure of the animal fabric as is ſufficient to balance the action of the ſolids, if there was no contrivance for dephlogiſticating the blood, the whole maſs would ſoon be rendered unfit for the purpoſe juſt deſcribed, as well as of communicating heat, and death would preſently enſue. Nature has therefore provided the animal with lungs ; the blood, phlogiſticated as already related, is conveyed to that organ ; the air in inſpiration reſtores it to its original purity by taking from it its phlogiſton, and furniſhing it in return with fire, and thus renders it again fit for the purpoſes of animal motion and heat. In proportion therefore as the ſum of the whole action of the fibres of an animal is greater, that is, in proportion as a greater quantity of blood is phlogiſticated in a given time, the motion of the blood ought to be increaſed, and the inſpirations of air more frequent, in order that the reſtauration of the blood to its former purity, may keep pace with its phlogiſtication in the body.

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* Senſation is not here conſidered.

Now, as life depends on the action of the fibres, as above, as there is a neceſſary connection or dependence between the action of theſe fibres and the phlogiſtication of the blood ; and as from the great number of moving fibres in the body in continual action, and the ſmall quantity of blood, the latter will be preſently phlogiſticated, we have an idea of the very great importance of reſpiration, and the abſolute neceſſity of it to the continuance of life, as we find by experience to be the caſe; neither the *heat of the blood*, nor even *the vital motions of the ſyſtem* being capable of exiſting long without it.

SECTION XII.

Of the Action of the Fibres, or muſcular Motion.

WHAT has been ſaid in the two laſt ſections may be allowed, perhaps, to be in ſome degree probable. I ſhall give no opinion with regard to what follows.

THE idea of muſcular motion, which I had formed to myſelf many years ago, was, that *by the influence of the nerve, the particles which compoſe a muſcular fibre had their attractive forces increaſed, ſo that they were drawn nearer together, but that as ſoon as that influence ceaſed, the increaſe of attraction vaniſhed, and the particles receded to their previous diſtance from each other.* I had contented myſelf with a theory in the abſtract; but Dr. Prieſtley's admirable diſcovery will, perhaps, enable me to aſſign the phyſical cauſe of this contraction.

It appears probable to me, after an attentive conſideration of the ſubject, that the matter or fluid contained in the nerves which ſerve for motion, is *the phlogiſton, combined in a coherent form with an earth already more intimately blended with a conſiderable quantity of that principle; ſo that their combination is but weak* *. Thoſe who have read the ſeventh ſection carefully will comprehend my meaning by this definition, and therefore I need not comment upon it. This matter does not ſeem to be derived from the nerve into the fibre of itſelf, or by propulſion, like the blood, for if the nerve be tied, it does not ſwell between the ligature and the brain. The matter of the voluntary nerves is, I think, only driven down by the will †. That of the involuntary ones is obtained by means of the pulſe of the arterial blood, and other ſtimuli in the body, by *irrita-*

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* Some phenomena ſeem to ſhew that the latter ingredient only is ſecreted by the brain, and that the former is imbibed from the ſtomach, &c. That the nervous fluid is not the electrical matter, as ſome have ſuppoſed, is plain from its not combining with the blood in the manner the phlogiſton in queſtion is found to do.

† Pain is a ſtimulus to theſe nerves; but then it is by its action on the ſenſory, &c.

tion, or *reflux*. Hence perhaps one reaſon * why the blood does not move in a ſmooth uninterrupted courſe in the arteries, but by pulſes; and hence alſo the reaſon that it does not flow by pulſes in the veins, the fibrous mechaniſm terminating where the veins commence, ſo that there is no further occaſion for it. The reaſon of all this ſeems to be, *that ſuch a quantity of matter only may be occaſionally derived from the nerves, as may be neceſſary for the purpoſes of the animal economy*, &c. which, therefore, is left to be regulated by the will, by the pulſe of the blood (the force of which depends on muſcular action), by heat, and other ſtimuli.

WHEN by the pulſe of the blood, the influence of the will, &c. a portion of this matter is derived from a nerve into a fibre, it ſeems to me that the particles of which the fibre is compoſed, having a greater attraction for the phlogiſton, than the earth has with which it is already combined, take the phlogiſton from that matter, and thereby have their force of cohe-

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* The other reaſon ſeems to be that the fibres may be put into vibrations, by means of which the effect mentioned is alſo probably produced. Of theſe vibrations I may ſpeak more at large in future.

ſion increaſed; the fibre, therefore, will contract: but the particles of blood flowing by the fibre, and having a ſtill greater attraction for phlogiſton, takes it immediately from the fibre, which therefore is again relaxed. Hence, as the contraction of the fibre is but momentary, if its contraction be required to be continued for a given time, there muſt be a continual derivation of matter into it from the nerve during that time *.

IT may be aſked, that if muſcular motion be performed by means of phlogiſton cauſing a temporary increaſe of attraction or coheſion in the particles of the fibres †, why this indirect method

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[*] I endeavoured to account for muſcular motion by the phlogiſtication and conſequent contraction of the blood only, and alſo by the æther diſengaged from the phlogiſton by the ſtronger combination. (Vide ſection VII.) But neither of theſe by any means anſwer to the phenomena. It may be obſerved that, probably, only the craſſamentum, or its coagulable lymph, attracts the phlogiſton from the fibres. That phlogiſton when combined brings particles nearer to each other is plain by its effect on air, metallic calxes, &c.

[†] If an artery be compreſſed, a ſenſation of warmth is perceived in the part which it ſerves; but as the blood returns, cold is felt. The warmth aroſe perhaps from the fibres being phlogiſticated,

method was adopted, and why the nerves were not furniſhed with it as a fluid, ſo that it might have been derived from them immediately to the fibres? It may be anſwered, that probably phlogiſton cannot be managed thus *per ſe*; and if it could, yet the quantity which a nerve would contain would not perhaps be ſufficient for a ſingle contraction of a muſcle: whereas, by this contrivance, a nerve can contain a ſufficient quantity to laſt a long time. But there is, probably, ſtill another reaſon; it has been an opinion of long ſtanding that the parts of the body are nouriſhed either wholly, or chiefly, by the nerves; for a part rendered paralytic by dividing a nerve waſtes, notwithſtanding

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phlogiſticated, and the blood not being able to take the phlogiſton from them, by which their attractions for fire continued diminiſhed. But when the blood flowed again, and attracted the phlogiſton from the fibres, cold muſt have been the conſequence, by the theory of combuſtion before explained. Alſo, when the artery only is compreſſed, the fibres ſeem to be more rigid or contracted than naturally. But when only the nerve is compreſſed, the fibres ſeem, on the contrary, to be more relaxed. If the experiments which I have made on myſelf (of which theſe concluſions are the reſult) can be depended on, they furniſh a kind of proof of the theory of muſcular motion above laid down. It is alſo known that a muſcle does not ſwell when it contracts.

that the blood flows through it as uſual. If the foregoing conjecture be true, the nervous matter is a compound of phlogiſton, and an highly phlogiſticated earth; and each of theſe ingredients may have their reſpective uſes. The uſe of the phlogiſton may be to cauſe the contraction of the fibres, and the heat of the blood. that of the other ingredient (the phlogiſticated earth) to nouriſh the fibres, &c. not perhaps alone, but conjointly with the blood; and hence the attraction of the fibres for phlogiſton is between that of the nervous earth, and the blood. Hence alſo the gelatinous nature of the fibres. Hence people who uſe no exerciſe have their fleſh more delicate and fat than thoſe who labour hard, the nervous matter of the former not being ſo liable to be carried off, but enters more into the compoſition of the fibres *, and ſome of it, perhaps, even in an undecompoſed ſtate. In the hands of a Pringle, or a Fothergill, theſe obſervations, and others which have been given, might, perhaps, be rendered of uſe in the practice of phyſic. It may be added, that as oil of vitriol cannot decompoſe char-

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* Hence the neceſſity of reſt, or ſleep appears.

coal, ſo blood may not be able to decompoſe the nervous matter itſelf, though it ſo readily takes its phlogiſton from the fibres. The nervous medulla is not eaſily combuſtible by *air*, if I remember right, notwithſtanding that it is ſo readily decompoſed by the moving fibres, if the foregoing conjectures be true.

It may alſo be objected, that a longer time ſeems neceſſary for this proceſs than appears to be conſiſtent with the inſtant contraction of a muſcle from the influence of the will. But not to mention how quick the tranſition of ſo ſubtile a principle as phlogiſton may be effected, I could, I think, clear up this difficulty by quotations from papers on the ſubject; but as that would lead me too far out of my way, I ſhall only obſerve, that the perception which we call *willing*, and which we uſually conſider as the cauſe of the action of a voluntary muſcle, is only an effect of the ſame cauſe in the ſenſory, by which the contraction of the muſcle is brought about. And, to illuſtrate the reſult by a ſimile, as when a man is ſhooting at a mark, and we ſtand near that mark at a diſtance from the man, the ſhots are heard to ſtrike againſt the paper as ſoon as the report of the gun; ſo, for a reaſon ſomewhat ſimilar, the volition and the action, ſeem to us to be in a manner cotemporary. It may likewiſe be remarked that our perceptions and actions are exceedingly ſlow, when compared with the action of the more ſubtile principles of nature, as I may hereafter explain, if the preſent work be approved.

A CERTAIN degree of heat, though neceſſary to a particular ſpecies of animals, is by no means ſo to the animal functions, or to animal life in general. Thus fiſhes are as perfect in theſe reſpects as quadrupeds, though their heat be much leſs. The heat is neceſſary to the liquifaction of the blood, and, perhaps, of the nervous compound; and alſo, to enable the fibres the better to decompoſe that compound, and the blood again the fibres. It may hereafter be ſhewn that it alſo probably aſſiſts in the vibrations of thoſe fibres. The blood of fiſhes is fluid with a degree of heat in which that of quadrupeds would be congealed. Animals which require much heat to keep their blood, &c. ſufficiently fluid, decompoſe a proportionally larger quantity of air, ſo that their blood may be more heated in the lungs, and alſo that its particles may carry a greater quantity of fire into the body to be extricated by the phlogiſton from the nerves. Fiſhes, whoſe heat is required to be but little, decompoſe a ſmaller quantity of air, in an equal time than quadrupeds; and the air which is ſeparated from the water by their gills, and again purified by water, may be ſufficient for that purpoſe. Now as leſs fire is ſeparated from the blood of fiſhes than from that of quadrupeds, it argues that leſs phlogiſton is alſo imparted to the blood by their nerves: and this agrees with an obſervation of phyſiologiſts, that *the fibres of cold animals are more irritable than thoſe of hot ones.* The balance therefore is preſerved; for as leſs *heat* is required to liquify their blood, &c. than in land animals, ſo leſs *phlogiſton* is neceſſary to the contraction of their fibres.

It would be eaſy to enlarge on ſuch a ſubject as this; but as I only offer what has been ſaid as ſpeculation, and by way of hint to be proſecuted by others, I ſhall not *here* purſue the idea any farther. I will only add a wiſh that what I have offered, may not give occaſion

ſion to the barbarity of making experiments on living animals. Were I even certain that this theory could be proved by making ſuch experiments, I would not attempt them, as I do not think we are by any means warranted in putting animals to torture to gratify philoſophical curioſity: I would not be underſtood as ſpeaking this from a principle of ſuperſtition; it is dictated by my own feelings: that man who has experienced in himſelf the extremity of *pain*, muſt be ſomething worſe than I can imagine to inflict it on animals, who are incapable by their natures of giving him cauſe.

APPENDIX.

SINCE the foregoing work was finiſhed, a book has been publiſhed on the ſubjects of animal heat, &c. by Dr. Leſlie. The very favourable account which the Reviews gave of that work, and the deference which I paid to the judgment of the authors of them, made me at firſt doubt whether I had not proceeded entirely on wrong principles in my inquiries on the ſame ſubjects, and had reſolved to withhold either the whole Eſſay on Combuſtion, or at leaſt that part of it which treats of reſpiration, &c. from public view; but after conſidering the matter more attentively, and comparing the different performances, I thought I diſcovered reaſons for imagining that my arguments were not ſo fallacious as I had at firſt concluded. I was at length ſo far ſatisfied of this, that I drew up a paper by way of refutation of Dr. Leſlie's theory, and intended to have ſubjoined it to my Eſſay; but after the work was in the preſs, another performance on the ſame ſubject was put into my hands, written by Mr. Crawford *, which contained experiments totally ſubverſive of Dr. Leſlie's principles, and therefore rendered my refutation, which was chiefly ſpeculative, needleſs.

WITH reſpect to the latter performance, I do not heſitate to pronounce it one of the beſt philoſophical pieces that the preſent age has produced. I have no knowledge whatever of the Author, any more than I have of Dr. Leſlie, and therefore can have no other motive for praiſing his work than a ſenſe of its merit: I read it with pleaſure, not only on account of the new and important points of philoſophy which it unfolds, but of the truly ingenious and philoſophical manner in which he has treated his ſubjects. We are here preſented with a ſpecimen of the true method of inveſtigation in philoſophical

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* I had not the pleaſure of ſeeing this excellent work till ſome time after it was publiſhed.

matters, and with an example of that becoming modeſty which always accompanies real genius.

THIS juſtice which I do to Mr. Crawford's performance may be conſidered as the more ſincere as I frankly acknowledge that the pleaſure with which I peruſed it was not unaccompanied with pain. I had been writing on the ſame ſubject; had advanced a ſtep farther than others had done, and was about to make my diſcovery public. Had I been fortunate enough to have gone to the preſs before the ingenious Author I am ſpeaking of, a ſmall degree of fame would probably have been acquired by the publication: but this Gentleman has outſoared me; he has completed the ſubject which I had only begun, and proved by facts, propoſitions which I had only offered as conjectures. Unhappily I have been ſo circumſtanced that it has not been convenient for me to engage in a courſe of experiments requiſite to the complete inveſtigation of a philoſophical truth. I could only argue from thoſe facts which had been publiſhed by others, and from analogy; yet by the help of theſe I was enabled to diſcover that combuſtion is a truly chymical proceſs,

ceſs, and that it depends on the ſuperior affinity or attraction between phlogiſton and air: that by the combination of theſe, a degree of heat is generated ſufficient to account for the heat and light of flame, and for the continuance of the combuſtion after once begun. But the origin of that heat I had only dreamt of; and even my dream I find did not wholly correſpond with the truth: yet had I publiſhed firſt, I ſhould perhaps have deprived our illuſtrious Author of ſome part of the glory which he has gained by his excellent performance: I ſhould at leaſt have had the credit of diſcovering the propoſitions eſtabliſhed in the third and three ſubſequent ſections of the Eſſay, and perhaps of furniſhing the *hint* of the remainder of the ſubject. but Mr. Crawford has fairly got the ſtart of me.

This, however, is not the only inſtance of two perſons unknown to, and ignorant of each others purſuits happening to hit upon the ſame diſcovery. That mine were made independant of that Gentleman's, appears by a view of the two performances; for it will be ſeen that we proceeded on a very different rationale, and that we arrived at the ſame concluſion by direct oppoſite roads. My learned and worthy patron, whom I have alſo the honour to call my friend, will do me the juſtice to acknowledge that my Eſſay was in his hands, and that I had agreed with the bookſeller for the publiſhing of it before Mr. Crawford's, or even Dr. Leſlie's performance appeared.

I AM weak enough to confeſs that I ſhould like to have had a ſhare in the honour of this diſcovery, and perhaps the candid in the learned world will not refuſe me ſome credit on that head, on a review of the evidence before them. Yet I ſhould not have publiſhed this Eſſay after having read Mr. Crawford's treatiſe, but that it was already in the preſs, and that there are a few points in which I had gone perhaps farther than that gentleman, or in which he does not ſeem to be ſufficiently clear.

THE general cauſe of combuſtion *, for ex-

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* It is worth while to obſerve that extraneous fire may be ſaid to heat bodies *poſitively*; phlogiſton, friction, &c. *negatively*.

ample, as far as relates to heat, is elegantly aſſigned by the learned author; and the different proportions of fire which he has ſhewn to be contained in fixed and atmoſpherical air, are far greater than I imagined *a priori* *. The light, the different colours of the flames, and ſome other particulars which he has not attended to, may perhaps be underſtood, in ſome meaſure, from my Eſſay.

"It is probable," ſays the ingenious author, "that the vapour of pure nitrous acid contains as much abſolute heat as atmoſpherical air; for the power of the former in maintaining flame is nearly as great as that of the latter. In the deflagration of nitre, the acid is converted into vapour, which being the ſame moment combined with the phlogiſton of the coal, the fire is inſtantly diſengaged, an elaſtic fluid is generated, and a loud exploſion produced." That *air* was really contained in the nitrous acid previous to the combuſtion *, is evident from the fixable air which is generated. For if the vapour had been merely of an aqueous nature, though it might have been expanded by the heat, it would have been condenſed into an aqueous liquid, and not fixable air, when cold †: and this being admitted, the Author's ſuppoſition, that "the vapour of pure nitrous acid contains as much abſolute heat as atmoſpherical air," will appear to be true, and is a farther confirmation of his excellent theory.

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* Had I meaſured the degrees of heat before, and during the combuſtion, and noted the quantity of air conſumed, this difference might in ſome meaſure, have been diſcovered? but through want of conveniencies, I was obliged to content myſelf with a ſolution in the *groſs*. The difference of elaſticity, &c. in fixed and atmoſpherical air, I confeſs, does not ſeem to anſwer to their quantities of fire, as diſcovered by Mr. Crawford; perhaps ſome of the ſuggeſtions at the end of the VIIth. and IXth. ſections will better account for it.

Mr. CRAWFORD has proved that phlogiſton and fire are different fluids, contrary to what has hitherto been imagined. I had attempted to ſhew that *æther* was a third fluid, different from both theſe.

OUR Author imagines the attractions of bodies for phlogiſton to be proportional to the degrees of heat neceſſary to begin their combuſtion,

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[*] Vide ſection VI.

[†] Vide ſection VII.

buſtion, and I once fell into the ſame error; but that this is not always the caſe will, I think, appear by what is ſaid on that ſubject in the VIIIth. ſection of my Eſſay. May not the differences there obſerved be partly owing to the different ſtates of the double affinity?

I HAD endeavoured to ſhew that phlogiſton diminiſhes the attraction of bodies for fire, in proportion to the force of its combination; and that this force of combination is capable of being intended or remitted even in the ſame body. But of the latter of theſe propoſitions, Mr. Crawford does not ſeem to have been apprized.

THAT Gentleman, in one part of his admired performance, has run a compariſon between fire and fixed air; but fixed air is not regulated by an equilibrium, or common temperature, like fire *, neither does it appear that fire exiſts in a looſe

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* Fire does not ſeem to cohere with, and form an eſſential part of bodies like phlogiſton, and fixed air. If the phlogiſton of a metal, or the fixed air of marble be taken from them, the nature and conſtitution of thoſe bodies are quite altered, or they are decompoſed; but this does not happen with regard to

a looſe or ſeparate ſtate like that fluid, but is retained by the particles of bodies according to a certain law, if my reaſoning be juſt. The affections of the ſenſe, and of the thermometer by heat, are not, I think, conceivable but by admitting that law. (Vide caſe xi. ſect. VIII.) When phlogiſton is added to air, the fire, according to my idea, is not *expelled* *, or does not *fly off* in the manner of fixed air from bodies; it would ſtill be retained by the air, though in a leſs forcible manner, if there were no other bodies near, or bodies by which it was not more powerfully attracted. If the bodies around were hotter, it would even attract fire from them; but this is ſpoken with ſubmiſſion to better judges.

In regard to animal heat, I find by our excellent Author's experiments, that I had fallen

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to their fire, at leaſt there does not ſeem to be the ſame kind of analogy as between phlogiſton and fixed air. There ſeems to be a greater analogy between electricity and fire in this reſpect.

[*] I have uſed this word in many places, but in ſuch a manner as to carry with it the above meaning.

into an error in imagining that the blood is partly heated in the lungs *. *That heat however is generated by the decompoſition of air in that organ*, appears from hence, that the air which is expired is hotter than that which is inſpired; and alſo by the following quotation from our Author's work. "By the heat of the ſurrounding medium, the evaporation from the lungs is increaſed. Now it may be ſhewn, that if the evaporation from the lungs be increaſed to a certain degree, the whole heat which is ſeparated from the air will be abſorbed by the aqueous vapour." And by the converſe, if the evaporation from the lungs be diminiſhed, the

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[*] Mr. Crawford in his firſt propoſition affirms, that air is fitter for reſpiration in proportion to the abſolute heat which it contains. But it ought to be obſerved that air is rendered unfit for reſpiration by other means beſides phlogiſtication; as by particles floating in it which irritate the lungs, and the like: thus, in combuſtion of ſulphur, the acid is as prejudicial in this reſpect as the phlogiſton. This conſideration does not ſeem to have been ſufficiently attended to of late; but aſthmatic people daily experience the juſtneſs of the remark. Pure fixable air does not kill by irritating the lungs, but by not carrying off the phlogiſton of the blood. The ſalubrity of air therefore cannot be determined by the eudiometer alone with ſufficient accuracy.

blood will be heated. But in general the evaporation from the lungs is ſo proportioned, that the heat of the blood is not increaſed in paſſing through that organ: my error aroſe from not attending to this circumſtance, which indeed could not have been known but by experiment.

The only inſtance, of moment, of our admirable philoſopher giving into hypotheſis is with reſpect to the origin of the phlogiſton imbibed by the blood; he ſuppoſes that it is taken "from the putreſcent parts of the ſyſtem." But I ſee no reaſon why the phlogiſton from theſe parts may not be diſcharged either wholly, or chiefly, by perſpiration, and by urine; and that it is ſo, ſeems apparent by the very great quantities which theſe excrements, eſpecially the former, contain. Neither does the neceſſity of the elaborate, and (to all appearance) very important proceſs * which he had been deſcribing†, on this ſuppoſition, ap-

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* Reſpiration, &c.

† Hence Mr. Crawford is at a loſs when he comes to apply his principles to cold animals.

pear. I may have been prepoſſeſſed in favour of my own hypotheſis, and therefore may not be a proper judge of this matter: as a proof of it, I ſhall not ſcruple to confeſs that I felt myſelf pleaſed on finding that Mr. Crawford, in this part of his work, deviated from my track; I ſhall therefore leave the merits of the two hypotheſes to be determined by the more impartial Reader, or by Mr. Crawford himſelf; for I have conceived ſo good an opinion both of his judgment and candour, from his admirable performance, that I would cheerfully acquieſce in his determination.

HAVING taken the freedom to point out anothers errors, I ſhould next proceed to enumerate my own. The taſk, however, would now not only be laborious, but uſeleſs, as my theory was only given by way of conjecture; though, for the contrary reaſon, it was proper to notice any error of Mr. Crawford. Some of my miſtakes I have already mentioned; the following, though not ſhewn to be ſuch by Mr. Crawford's experiments, were yet diſcovered by a more ſtrict review of the foregoing Eſſay in conſequence of that Gentleman's very ingenious publication.

In the VIIth. ſection I adopted Dr. Black's ſuppoſition, that *phlogiſton has a centrifugal tendency*, and thought I had accounted for it by imagining that its particles attracted æther. But though this ſhould be allowed, yet unleſs the globe of the earth did alſo, there would be no repulſion between them. Particles of earth gravitate becauſe æther is mutually repelled by them, and by the terreſtrial globe; and, therefore, if phlogiſton attracts æther, there can, at moſt, only an indifference be produced in it with reſpect to gravity or levity, the globe of the earth, and the particles of phlogiſton mutually deſtroying each others effects. The rays of light, ſetting aſide their *inflection*, which may be otherwiſe accounted for, do not ſeem to have either centripetal or centrifugal tendency; or if they have either it does not appear to be in any conſiderable degree: the like may be obſerved of electricity, which I take to be phlogiſton in the next degree of purity to light. If fire attracts æther, fire alſo muſt be alike indifferent

ferent with regard to gravity or levity, and this ſeems, by experience, to be the caſe. There are however, methods of conceiving how phlogiſton may diminiſh the gravity of bodies; or do metals, &c. attract air, when calcined, in lieu of their phlogiſton, and thereby have their weight increaſed? for an effervеſcence attends the reduction of thoſe calxes *, as I find by the Chymical Dictionary: ſhould this latter be the caſe, it would appear (and it would be a little extraordinary) that *error* had led to me *truth.*

THE above, as it has accidentally been the ſource of right, ſo it has alſo of wrong reaſoning, as may be ſeen in the courſe of the VIIth. ſection. I do not however find reaſon to reject the general ſyſtem ſuggeſted in that ſection; and am ſtill inclined to think that *Æther*, *Fire*, *Phlogiſton*, and *Earth* are the four principles of which the world is compoſed, (taking alſo into conſideration what was ſaid of their properties in page 127): but the following ſeems to be a more proper arrangement of them, and of their uſes. Æther, and earth, are mutually repulſive; hence the gravitation of the particles of the latter: fire and phlogiſton are principles intermediate to thoſe; the latter ſeems to be the principle of coheſion among the particles of earth; the former of their ſeparation. Phlogiſton is alſo the cauſe of light, fire of heat, and on the various compoſitions or aſſociations of the above principles, the ſenſible qualities of bodies, and the phænomona of nature in general, ſeem to depend.

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* Dr. Prieſtley ſhews that dephlogiſticated air may be expelled from the calx of lead. The dephlogiſticated air may have been formed of fixed air which the calx had attracted, and afterwards decompoſed. (Vide § VI.)

As I formally renounce the falſe reaſonings which may be met with in the VIIth. ſection, and ſome other parts of this Eſſay, candour, I preſume, will prevent their being brought in judgment againſt me.

If any conſiderable part of the foregoing work ſhould have the good fortune to be approved, the errors which I have diſcovered, and any others which may in the mean time appear, would be omitted in a future edition. They had certainly been ſo in this, as well on my own account as the Reader's, had I ſeen Mr. Crawford's performance in time. The greateſt philoſophers that ever lived have fallen into errors *, eſpecially where experiments where wanting to aſcertain the truth; it is no diſgrace to err in ſuch good company, and as I make the critic my *prieſt* by confeſſing to him my *faults*, I have a firm *faith* in his ſupporting the chriſtianity of the character by granting me *abſolution.*

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* The recent inſtance of Dr. Leſlie on the ſame ſubject, a gentleman who had every advantage over me in point of information, might be urged; and alſo the opinion of the gentlemen concerned in the Reviews on his performance.

P. S. It

P. S. Does not Dr. Prieſtley's diſcovery of "light decompoſing fixed air in water." depend on the principle laid down in the XVth. caſe, and applied to friction, percuſſion, &c.* at the end of the IXth. ſection? are there not in the water particles, either of the water itſelf, or of more fixed ſubſtances, which have originally ſtronger attractions for phlogiſton, than the particles of air? do not the rays of light, by their action on thoſe particles, increaſe their attractions for phlogiſton, and thereby enable them to take it from the particles of air contiguous or perhaps in combination with them, agreeable to the principles above alluded to? hence heat has not this effect. It ſeems therefore to be analogous, in principle, to the decompoſition of fixed air by agitation with water, a former diſcovery of that excellent philoſopher.

I HAVE ſuppoſed in page 195, that the phlogiſton is derived to the nerves, not from the brain, but from the chyle in the various parts of the body; as the chyle is agitated in and againſt the veſſels, may not this be brought about on the ſame principle? Dr. Leſlie advanced a propoſition, that "by the action of the veſſels, the phlogiſton of the chyle is gradually evolved throughout the body." There ſeems to be ſome truth in the opinion, though that Gentleman, imagining phlogiſton and fire to be the ſame, erred in the conſequences which he drew from it. If the above be the true ſtate of the caſe, and if what is advanced in the three laſt ſections of my Eſſay be juſt, the phlogiſton is transferred from the chyle to the nerves, from the nerves to the fibres, from the fibres to the blood, and from the blood to the air. Some other operations of nature may probably depend on the ſame principle.

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* That is, to the putting the particles of bodies into vibrations; though the heat is not the immediate effect of theſe vibrations, as has been imagined, but as explained in ſection IX.

TORN §

THE END.

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